THE LANCET.

VOL. II.] LONDON, SATURDAY, APRIL 30, 1836. [1835-36.

CLINICAL LECTURES AND REMARKS,

[DELIVERED IN 1836, ON

DISEASES OF THE SKIN, VENEREAL DISEASES, AND SURGICAL CASES,

AT THE

SKIN INFIRMARY,

AND AT THE

JERVIS-STREET HOSPITAL, DUBLIN.

BY

DR. WALLACE,

Surgeon to those Institutions.

REMARKABLE INJURY OF THE BRAIN.

Ward No. 1.—*April* 6. *Andrew Bolger, aged fifty years, a painter.*—Direct your attention, gentlemen, to this case. It is one of interest. This man was thrown, three days ago, from a scaffold, while painting the staircase of a house. He fell, I understand, on his vertex, and, to use the expression of the bystanders, "his head was driven into his body." Yet, you observe, he has no wound, nor any tumour on his head. He has merely an abraded surface of the scalp, corresponding to the middle of the sagittal suture. Remark, he has either a peculiarly formed neck and back, or some injury has been done to the spine, for there seems an unusual depression in the back of his neck and between his shoulders, and I am informed that on his admission he appeared to suffer much from pain in his neck, and from difficulty of swallowing. He was not, I understand, rendered senseless by the fall; he was merely stunned. Nor had he any vomiting. I am also told that he was conveyed to the hospital on a car half an hour after the accident, that he got off the car himself with some little assistance, and that he walked up the stairs of the hospital to his bed. On admission he was cold and appeared drowsy, and some time after being put to bed, he had two general convulsions.

Mark well the symptoms under which he now labours. He appears like a person half asleep. You can rouse him by speaking to him, or by shaking him. He then answers you rationally, but in a whisper. This whispering state of voice has existed only for two days. He lies prostrate on his back, and is unable to sit up. When you raise him, he falls back again. His discharges escape involuntarily. His eyes seem heavy, or as if filmed, but the size and motions of his pupils are natural. His countenance is not flushed. His respiration and pulse may be said to be little altered; they are perhaps laboured.

These are a very compound set of symptoms. If you ask me to class them, I should not feel myself able to do so. There are some of them referable to concussion, some appear to arise from an oppressed brain, and some denote mischief to the spinal marrow. Here you see a proof that you must learn your business at the bed-side of the patient, and not in books. Do not suppose, however, that I undervalue books.

What can be done? What is to be most feared? There are several sources of anxiety. There is, as I have said, reason to fear that the brain is compressed, that the spine is injured, and that part of the symptoms are owing to concussion, or to a general shock of the nervous centres. I do not know that we can do anything more than guard against inflammation, and diminish the effects of pressure, by emptying the vessels of the brain. His pulse will bear bleeding. Let him be bled largely, and let his bowels be again well emptied. His head has been shaved, and as its temperature is rather over *par*, let the cold lotion be applied to it.

(*Same Ward, April* 7. *Same Patient.*—This man does not seem much worse. The same symptoms, however, continue, and his pulse is weaker. Let a blister be applied to the occiput, and let him be put under the influence of mercury.

(*Same Ward, April* 8. *Same Patient.*)—This man is rapidly losing ground. His pulse and respiration are more frequent. You can also detect an irregularity in the pulse. He is more insensible, his breathing is more laboured. The nurse informs me

that he occasionally wanders, or talks incoherently, but the general convulsions which he had on the day of his admission have not returned. It appears to me that the symptoms of compression are more decided, and that those of inflammation are setting in. Remark how rational he still appears when you rouse him, and the very low whispering tone in which he replies to our questions. I do not think we can do anything more for him than we are doing.

(*Same Ward, April 9. Same Patient.*)—He is dying; feel what an alteration in his pulse. How weak and rapid it has become! It is not less than 130. Observe how laboured and how sonorous his breathing is. It is forty-eight in a minute. He can no longer be roused. He has lost the power of swallowing. Look at the curious motion of his lips and cheeks during respiration. His cheeks are alternately drawn in and puffed out in inspiration and expiration. I presume he cannot survive this day. The film of death is, as it were, on his eyes, yet his pupils are sensible to the light of this candle.

Autopsy.—Remarks made in the Operating Theatre on the same Case, April 12.—We had, on Saturday, a most interesting examination of the body of poor Bolger. I regret exceedingly that so few of you were present. It would have given me much pleasure to have postponed the dissection until this morning, but if I had attempted to do so, as Sunday intervened, I should have lost the opportunity of making any examination. The body would have been carried off by force. I will tell you what I observed on dissection, and with the assistance of these *spoils*, and this drawing, which I have had made, I will endeavour to make you acquainted with all the morbid appearances.

The exterior of the body presented little that was remarkable. The face was pale and collapsed-looking; there was a bruise on one of the hands, and I thought, on passing my finger along the spinous processes of the vertebral column, which appeared much sunk in the neck and upper part of the back, that I felt a crepitus, but of this I am not certain. When the integuments were removed from the head and along the vertebral column, we found the cellular tissue under the scalp, and corresponding to the vertex and occiput, but particularly in the region of the mastoid processes, infiltrated or stained with dark-coloured blood. Blood was also effused among the muscles of the spine, so low down as the middle of the dorsal region; and the spinous processes of several of both the cervical and dorsal vertebræ were fractured at their junction with their laminæ. The whole spine seemed also unnaturally concave behind, or as if it had been over-extended.

On removing the spinous processes, and the laminæ of the vertebræ, a patch of coagulated blood was found on the outer surface of the theca of the spinal marrow. There was also much reddish serum in its cavity, but the spinal marrow itself was uninjured.

So far appearances corresponded exactly with the account given of the fall. The spinous processes of the vertebral column could not have been fractured, and blood effused on the surface of the theca, without the spinal marrow suffering,—therefore we shall feel little surprised that symptoms of such injury existed. Indeed, the surprise is on the other side, for I can scarcely reconcile the trifling symptoms of spinal injury which did exist before death, with the appearances of severe spinal injury presented on dissection. I have been told that the man walked up the stairs of the hospital after the fall; I have also been told that subsequently to his reception into the hospital, he got up from his bed, and went to the water-closet, and that to nearly the period of his death he moved his limbs through the bed, as if he had full power over them. I cannot deny these assertions, after the testimony which I have had. I conclude that the facts afford an additional proof to the many on record, of how far we are removed, as yet, from a satisfactory degree of knowledge of the pathology of the nervous system. There is no fact better ascertained than that the same degree of injury of the nervous centres produces symptoms of very different degrees of severity in different persons.

I should have noticed to you, when speaking of the appearances presented by the exterior of the body, that the cuticle was removed from the sacrum and back part of the nates, from those parts, in fact, which were pressed on in lying. This is worthy of attention. You know the patient survived the injury only a few days. How did it happen that he began so soon to strip? The injury of the spinal marrow explains this. In all cases of loss of energy of this organ, the parts supplied by its nerves, lose, to a great extent, the power of resisting the irritating influence of mechanical and chemical agencies. Hence the strippings, the gangrenes, the ulcerations, which are the inevitable lot of the unfortunate persons who have experienced severe injury of the spine.

To return to the dissection. Upon removing the calvarium, it was found separated into two nearly symmetrical halves; and, on further examination, it appeared that this was owing to a fracture having taken place, which extended from the middle of the frontal bone, backwards through the sagittal suture to the foramen magnum; yet this fracture was not observed until after death. The surface of the dura mater presented, when the skull-cap was removed, this remarkable appearance. (Dr. Wallace showed a preparation and a drawing.) This, you observe, is the longitudinal sinus. You

see it has been laid open, from its origin on the frontal bone to near its end in the lateral sinuses, and you see its area seems to be filled with coagulated blood. It is opened in almost as straight a line as if it had been cut with a pair of scissors. I have now to tell you of an almost unaccountable occurrence. This spicula of bone, which is more than half an inch long and an eighth wide, at its widest part, lay embedded longitudinally in the sinus. It is also remarkable that, although the sinus was thus torn, there was no blood effused on the surface of the dura mater. Did any portion of the blood, which I have told you stained the subcutaneous tissue of the scalp, come out of the cavity of the cranium? I think it did. The edges of the fractured bones were stained, as if they had allowed blood to ooze out between them.

How are we to account for this laceration of the sinus? How are we to account for the existence of the splinter of bone which lay in the sinus? I think we are compelled to believe, that at the moment of injury, a very considerable degree of locomotion of edges of the fragments on each other must have taken place, and a consequent momentary alteration of the figure of the skull. The fracture was so extensive, that a great range of motion must have been permitted between the two sides of the cranium. Admitting the occurrence of this motion, we can easily conceive how the superior wall or parietes of the longitudinal sinus may have been torn, and how a portion of the edge of the fractured bones may have been detached, and then buried in the sinus.

Upon the removal of the dura mater, the entire surface of the cerebrum and cerebellum, but particularly that of the left hemisphere of the cerebrum, presented the appearance which you see in this specimen, and in this drawing made from it. The convolutions, you may remark, appear painted with blood, and this blood is effused into the pia mater, and under the arachnoid. Although it appears as if it could be rubbed off, you see it cannot. It is a kind of ecchymosis, and is removed when we remove the pia mater and arachnoid. There is also, in parts, considerable sub-arachnoid effusion, indicative of inflammatory action.

On raising up the front lobe of the left hemisphere, a large gush of blood escaped, and on examining from whence it came, it was found to have been lodged in a cavity fully the size of a hen's egg, seated immediately under the pia mater, in the medullary substance of the anterior lobe of the left hemisphere. Here is the cavity, and here is a drawing of it. The cerebral substance forming the walls of the cavity is, you see, in a very softened or pulpy state, and mixed with dark semi-coagulated blood. On the right hemisphere there was an effusion of blood of the same kind. Here is the part, and here is a drawing of it. This second cavity was not, however, you observe, larger than a nutmeg. There were also scattered in other parts of the medullary substance, two or three very minute spots of coagulum. The substance of the brain did not feel either unusually firm or unusually soft. There was no water in the ventricles, nor was the cerebral tissue unusually vascular. When the brain was removed from the cavity of the cranium, fully two ounces of blood, or bloody serum, were found in the base of the skull. Now

Can we explain the Connection of all the Symptoms with the particular Lesions discovered on Dissection?

We can account for death, but I fear our knowledge of the pathology of the nervous system, is not as yet sufficiently advanced to explain the cause of all the particular symptoms. For example, how did it happen that the eyes appeared so free from any participation in the injury? How are we to account for the rationality of the patient, almost to the day of dissolution? How can we explain the whispering character of his voice? This state of the vocal organs seems to be often connected with lesions of the anterior lobes of the cerebrum. To what was owing the peculiar action of the buccanter muscles, which occurred some time previous to death? It seemed to me to depend on a paralytic state of his lips. They remained constantly applied, and even glued, to each other; hence there arose an obstruction to the ingress and egress of air through the mouth. How can we account for the power which the patient possessed over his voluntary muscles, and which enabled him to get out of bed after he had lost the power of retaining his discharges, and when the organization of the parts supplied by spinal nerves, was so much enfeebled that they were not able to bear the pressure of the bed, or the irritation of the excretions. Should it be said that the involuntary discharge of the excretions depended on the state of the contents of the cranium, rather than that of the spinal marrow, we have still a proof in the stripping of his hips, of the lesion of this organ. To what was the absence of complete concussion in the first instance owing, when there was such a demonstration that the whole nervous centre, both that in the cranium and that in the spine, must have suffered so severe a shock? Did the fractures break the shock? How did it happen, that although the right hemisphere was vastly more injured than the left, there did not appear symptoms of greater injury at one side of the body than at the other? I could much increase these queries. What is the use of telling us how little we know on the subject of injuries of the brain, you may say? If it will have the effect of stimulating you to endeavour to

add to our knowledge by every means in your power, it will have a valuable influence. Oh, what ample opportunities are there not of investigating this most interesting subject! How advanced might our knowledge be respecting it, if we had been careful to register facts! but unfortunately for the advancement of science, the registry of insulated facts is, to most persons, an irksome occupation.

Let the appearances discovered on dissection in this case, make a deep impression on your minds. Why? you may ask. Because they teach a

Lesson, which may be of infinite value to you in Legal Medicine.

Remember that this man walked, after the receipt of the injury, up the stairs of the hospital. Remember that he spoke, that he appeared rational for several days, and reflect on the great injury his brain must have suffered. If you had not seen this case, or analogous ones, could you conceive such an occurrence possible? Could you conceive that a man could survive one minute such an accident? Suppose you were called to examine at an inquest a body which presented marks of violence about the head, equal to those presented by this case; and suppose that your testimony was required, whether an individual who had received such an injury could have walked any distance afterwards, would you not say, if you had not seen such cases as the present, that it was impossible? Reflect on the common consequences of even a trifling shock to the brain; reflect on the degree of injury which this man's brain must have received, and you will feel convinced how difficult it is to conceive, that locomotion or volition could have been performed after its infliction. Your testimony respecting the question, whether a man could survive such an accident for even a short time, and whether he could perform the acts of volition, might involve the ques- of life or death. [*See a lecture by Dr. Wallace on Wounds of the Heart*, LANCET, 1833-4, *vol.* 2, *p.* 140.] Suppose, for example, that the exculpation or conviction of the accused, depended on the possibility of the injured person having been able to remove himself after the injury; and suppose that your want of experience in cases like the present, on one hand, and your knowledge of the delicacy in general of the brain on the other, should have prompted you to deliver an opinion that such power of locomotion would be impossible. You see what the consequences might be. I do not picture to you an ideal case, I have known such cases. I have known such a case within the last few days. Let me tell you the heads of it.

The helper in a gentleman's stable was found dead in the coach-house in the morning; the gentleman came to me to know what should be done; I went to the place where the body lay, it was still in the coach-house; it lay on a wad of straw; the face had an apoplectic look: it was said that he had been intoxicated, and that by lying on his face he had been smothered. I however recommended that information should be given of the transaction at the police-office, for the account which the coachman, the companion of the helper, gave, seemed to me not to be very reconcilable in every particular with what I observed. There was, for example, a slight wound on the right eye, and a bruise on the temple. My advice was followed; an inquest was directed by the magistrates; the body was examined, and as soon as the scalp was removed, a most extensive injury of the head was discovered. The right side of the cranium was extensively fractured; the piece of bone which I show you, and which measures about half an inch in every direction, was driven in on the brain. It is a portion of the great wing of the sphenoid bone. It is remarkable that, although this piece of bone was driven in upon the brain, there was no wound of the skin over the depression. You therefore see that it is possible for an insulated portion of the cranium to be depressed, when there is no external wound. The middle artery of the dura-mater was opened, several ounces of blood were effused, and had coagulated between the bones and dura-mater, and caused such compression of the brain, that when this organ was exposed, its surface presented a fossa, in which the back of a hand, having its form made slightly convex, could be lodged.

Such being the state of the brain, the cause of death was evident, but what produced that state was the question. It was proved at the inquest that the deceased and the coachman had been quarrelling the preceding day; that there had been blood observed on some part of the dress of the coachman on the same evening, and that he had concealed for some time the state of the helper. Under these circumstances suspicion of murder fell on the coachman, who in his defence represented, that the helper received the injury by a fall from the horse. It appeared however, that after this fall, to which his death was attributed, he had gone into and drunk at a public-house. Under these circumstances, it was most important for the ends of justice, that the medical witness should give his assistance in determining the question of the manner in which the injury had been produced, and whether it was likely that after so severe a lesion, the deceased could have gone to a public-house and drunk, and subsequently returned to his stable. You seem to be impatient to know what opinion I gave. Well, I will tell you. I did not give a decided opinion; I could not. I said it appeared to me unlikely that the injury had been in-

flicted by a fall from the horse, for two reasons. 1st. The part of the head injured was not the part which most probably would have come against the ground, or against any body on the ground, nor was it in the situation in which a *contre coup* would have produced so severe an injury—for it was rather low in the right temporal fossa, and was more likely to have been produced by a heavy body rolling over the head, or pressing on the head, than by any other cause. 2nd. That the degree of injury was such, that it was unlikely, though not impossible, that the deceased could, after its infliction, have gone and drunk in a public-house. Now you see clearly from the case of Bolger, before you, that I could not have given positive testimony on this latter point; and had it not been from my acquaintance with analogous cases, I might have arrived at a positive conclusion, and thus have added very much, and perhaps unjustly, to the circumstantial evidence against the coachman. He has however, I understand, been committed for trial. Let me observe, that on the occasion of this inquest, as of innumerable others which I have witnessed, the want of medical knowledge in the coroner was severely felt. I trust, in these reforming times, a law will be introduced rendering medical knowledge indispensable in every coroner. In this respect, our criminal code is most deficient, and I have no doubt that the ends of justice are often defeated in this country from the want of a tribunal, properly qualified to determine medico-legal questions.

NORTH-LONDON HOSPITAL.

CLINICAL LECTURE

ON

FRACTURES OF BONES,

DELIVERED APRIL 15, 1836.

BY MR. LISTON.

I SHALL to-day, gentlemen, make some remarks upon a subject of very considerable interest—that of fractures. In the course of the summer months I shall direct your attention to the particular fractures as they occur, applying to them the principles which I shall now lay down with a view to inculcate a proper and successful practice in their treatment.

The existence of fracture is generally pretty manifest, although, in some situations, a good deal of care is required to ascertain the exact seat of injury, and to distinguish a fracture from a luxation. In some cases the existence of fracture is certainly not very apparent. We have a patient in the hospital who came here, stating that she had sprained her ankle badly two days previously; and you will recollect, it was only by pretty attentive manipulation that we discovered that the lower part of the tibia was broken, the fibula being entire. It was only by moving the parts about a good deal, that the crepitus could be discovered. (The lecturer here described the general signs of fractures of the bones, their cause, and the mode of detecting their directions. Respecting one cause he made the following remarks):—

Fracture from Muscular Action.

Bones, although in the most healthy state, may be broken even by the action of the muscles. We have had several instances under treatment, where the injury occurred in that way. The femur, and the humerus, are occasionally thus broken, and the patella is more frequently broken thus than by direct violence. This accident is more liable to occur from this cause, when their tissue is degenerated, when they contain a great deal of fatty matter, and when their shell is thin. We had a very good example of this accident in the case of Susannah Elgie, aged forty-nine years, who was admitted, some months since, on account of fracture of the humerus. That fracture occurred merely in the act of stretching out her hand to lay hold of one of her children—not to restrain its motions, but merely to play with it. You could scarcely suppose such an injury to arise from a more simple cause. I may recal to your recollection, that she had previously broken the clavicle, without almost being aware of it. That patient, however, had laboured for a very long period under some affection of the spine, in consequence of which she had been confined to bed. She laboured also under tumours in various parts of the body. She had a carcinomatous tumour of one mamma, well developed. It had not become an open cancer, but the lymphatic glands were affected. The tumour was fixed to the muscles, and, I should suppose, to the ribs. The other mamma was in progress towards the same condition. She had, besides, a tumour, of a hard stony consistence, behind the angle of one jaw. We were afraid, in the first instance, that the fracture had arisen from a malignant degeneration in the medullary canal of the humerus. You are aware that such malignant disease of the bones does occur. When the constitution is thoroughly pervaded by the carcinomatous diathesis, some part of the osseous tissue occasionally does take on this diseased action. The shell expands, and becomes exceedingly thin; at last the bone is almost entirely converted into soft matter, becomes completely degenerated, and gives way from the application of slight

force or muscular action. You will find, upon looking over my collection, many bones in which disease has made progress in this way, in which, after the fracture, the tumour has attained a great size; and these are cases in which the patient must ultimately consent to part with the limb, as the only chance, sometimes a slight one, of averting a fatal termination. In the case of the female patient, however, to which I have just alluded, we were agreeably disappointed to find the healing of the fracture go on just as well as under ordinary circumstances. There was a good deal of swelling on first undoing the splints, this disappeared, the bone became perfectly united, and she left the hospital much sooner than could reasonably have been expected. Her bones I dare say were not in the most healthy state for uniting, though they were, without doubt, in a very favourable condition for being broken; they had become loaded with fat, in consequence of long confinement to bed, and the shells had become thin. Malignant degeneration was not the cause of the fracture, as the result showed; had it been so, no union could reasonably have been expected to take place. The lecturer then explained the pathology of reunion in fracture, making, at the close of his observations, the following remarks:—

After a time the blood which was extravasated is reabsorbed; the reabsorption of the colouring matter, at all events, takes place; the lymph becomes organized, and in that organized lymph or fibrin the bone is found. The deposit of bone speedily takes place in the medullary canal. There is a sort of broad ring formed, which surrounds the fracture, at least when no displacement is allowed, and a kind of plug is formed, in the medullary canal, of new bone. The periosteum in such a case is thickened, but not converted or transformed into bone. You do now and then find plates of bone in the periosteum, but that occurs where there has been great laceration of the parts, where the broken bones have been allowed to remain over-riding one another, and where a greater degree of irritation has been permitted to be kept up, than was at all necessary. In the union, the swelling and deformity, the "exuberant callus," as it has been denominated, is occasioned in a great measure by the projection and overlapping of the broken ends, and also by the profuse deposit of new and organized material to which the displacement and consequent excitement of the surrounding parts give rise. Irregular portions of bone will, under such circumstances, be found in the intermuscular cellular tissue; and, in fact, from the relations of the divided ends, much more new matter is required to fix and consolidate the fracture than when they are more favourably placed; then, indeed, the swelling around the fracture is slight, and soon disappears. You find in many cases

Osseous Matter lying in the Cellular Tissue,

or attached to muscles or tendons; but I have my doubts whether that is deposited from the vessels of the cellular tissue or the muscular substance. It would appear, that from the vessels of the periosteum, the new bone by which the union, both temporary and permanent, is effected, becomes deposited after they have entered into the original bone. The osseous matter which is found in the cellular tissue has always appeared to me to be furnished by the vessels of splinters which have been abstracted from their original position, and which have in their new situation formed fresh vascular connections, and have retained their vitality. I now show you a piece of a tendon taken from a limb which was removed many weeks after a bad fracture; the tendon is that of the tibialis posticus, and you will find attached to, and firmly incorporated with, a large piece of bone. The opening in the periosteum had closed thoroughly, and a stray piece of bone derived its supply from the vessels of the tendon. If you look narrowly to it, you will find on the outer side a quantity of new bone growing out from, and ultimately connected with it, showing that it received its due supply of blood, and was perfectly alive. In that way you can account for the growth of new bone away from the natural situation of bone. The discovery of osseous tissue amongst the soft parts in fracture, has given rise to the notion, that all the parts about a fracture contribute to the formation of callus. It is found, that after the broken parts have been consolidated and the cure has approached completion, both the new and the old bone which does not contribute to its strength and solidity will be taken up by the absorbents, if the fragments retain their vitality; but if they be driven from their natural position, do not receive a supply of blood from the neighbouring soft parts, and become dead, an abscess will be formed, and those portions of bone will be thrown out; because, as I have already stated to you, dead bone cannot be, and is not, acted upon by the absorbents. The sequestrum in necrosis is not acted upon after it has been fairly separated from the system, as I mentioned formerly in a lecture, and tried to combat the notion that it was. It is a

Generally-received notion that dead bone is absorbed;

and it is thought by some, that the absorbents act very speedily on it, producing those worm-eaten appearances which you observe in sequestra. It has been asserted, that if you tie a piece of bone down upon an ulcer, the bone will very speedily diminish in bulk

and weight. It is said that Sir WILLIAM BLIZARD made some experiments of this sort, and that such was the result. I can only say, that even if such experiments were fairly made, they would not prove that sequestra are absorbed. In such experiments the dead bone must be exposed to heat, moisture, and the action of the air, and from putrefaction it may become altered in composition, appearance, and volume; but as to its being taken up by the absorbents, that is altogether out of the question. I am, however, in a situation to set this question pretty much at rest. A friend of mine, of whom I spoke to you before,—Mr. GULLIVER,—a very close observer, a careful experimenter, and a gentleman who has paid a great deal of attention to all that has been ascertained in pathology, writes to me to this effect on the subject: that he had made numerous experiments about the absorption of bone, and the result, so far as these trials went, was conclusive, that dead bone did not admit of removal by the absorbents. In some instances the pieces of bone were not weighed, but the following very interesting experiment I shall take the liberty of reading to you:—"Two sections of the tibia of a rabbit were introduced into the medullary canal of the same bones in the living animal, and retained there for upwards of six weeks; these portions of bone were accurately weighed by Dr. DAVY both before and after the experiment; one was of exactly the same weight as at first; the other had gained one-tenth of a grain, probably from a little albuminous matter which was not got rid of by drying, a circumstance the more likely, as the first was a hard compact bit of bone, and the second was light and spongy. The foreign bone was found surrounded with a bed of vascular soft substance, like coagulated lymph, and the same matter occupied that part of the medullary canal which was not filled up by the dead bone. In some instances, indeed generally, thinning and then ulceration of the walls of the old bone took place near the most superficial point of the foreign body, and opposite this, the growth of the soft vascular matter was so luxuriant, as to push away the dead portion, which would thus have been expelled if the animal had lived." Now he promises to repeat these experiments again and again with great care, and to let me have an opportunity of examining the products. I mean very shortly to turn my attention to this subject, and by next season I dare say, from my own labours and the contributions of my friends, I shall be able to show you something satisfactory. In the mean time you may safely take it for granted that bone, accidentally separated from its natural position, so long as it retains its vitality, will be acted upon by the absorbents, subject to the laws of vitality, but not afterwards.

Period of Consolidation.

The substance which is interposed between the ends of broken bone, supposing you bring the ends accurately together, remains soft for a long time. The temporary deposit—the callus outside—is formed in the course of a very short time. It remains soft for a little, but at the end of six or eight weeks—sometimes earlier, it becomes so firm that the limb (supposing the fracture to have taken place in one of the bones of the lower extremity) can support the weight of the trunk. At the end of five weeks even, we had a man who walked perfectly well after a fracture of the lower third of the femur. It was an experiment to be sure, but he persisted in it, and the bones supported him without any deformity occurring. The broken ends of the bone at that time could only be kept together by that external callous ring, and the plug in the medullary canal. These bands of union also remain even in healthy subjects soft and pliable to a certain degree, four, five, or six weeks after the injury: more so when the broken ends have not been kept very steady, have been frequently handled and *reduced*, being displaced first of course; this you will do well to recollect. You will not permit all patients with fracture to begin to use the limb quite so soon as the one I allude to did, otherwise you may find that a limb, apparently well cured, and tolerably straight, may become, from the action of the muscles, and the too early imposition of weight, as crooked as a ram's horn, and of an inconvenient brevity. Purmannus gives one exceedingly instructive case of this kind, in which a hangman's wife figures prominently. The limb was well cured by a surgeon, according to his account. The patient was a gross, fat, lazy man. The lady treated him with oils, which shrunk his sinews, and the limb got so short, that the toes only reached the ground. The surgeon was blamed, and sued at law. Purmannus blames the patient, and "the nasty filthy jade of a woman." By recollecting that the callus is long soft, you may, by gradual and well-applied pressure, fashion distorted limbs more handsomely than could well be supposed.

The substance interposed betwixt the broken ends, in time becomes more consistent and fibrous, ultimately it is converted into bone, and then the cure is completed by a "definitive callus," as DUPUYTREN has called it. This callus is exceedingly long in forming in some cases; and I have here two preparations of ribs which were broken rather more than two months previously to the patient's death. They are connected by bone on one aspect, but only by a dense ligamentous substance upon their concave side; the substance betwixt the broken ends is still very soft, and resembles fibro-cartilage. I

have here also a drawing of a bone that had been broken 110 days before being obtained, where the union was not complete.* Here, as you perceive, the broken extremities are still disconnected. There is no osseous union whatever. There is a shell on each side, adhering to the ends of the bone, and the medullary canal is obliterated, but still at the end of 110 days no permanent callus had been formed.

After the permanent callus is formed, the temporary callus is taken up; the medullary canal of the bone becomes again cellular, and assumes the same condition as previous to the fracture, and the hard swelling which surrounds the injured part, and renders the limb unshapely, and retards in some measure the muscular action, disappears also.

It would be out of place here to say any thing of the odd notions entertained by different authors regarding callus. It appears to have received a good deal of attention from DUPUYTREN, but it was very well known before his time. If you will turn your eyes to this drawing, you will be prepared to believe with me that the person to whom it belonged, who planned and executed it, knew something of provisional callus, and how it was formed. It was one of the sketches or plans of the late Mr. John Bell, the author of the admirable works on Wounds and on the Principles of Surgery. Blumenbach I think has also described both the ring surrounding the bone and also the obliteration of the medullary canal, the provisional callus in fact.

Now from circumstances of one kind or another, we find occasionally that the process of union is incomplete—it is not carried forward to a satisfactory conclusion—it stops short. The bones may be connected together, but then the substance which connects them together remains of a leathery consistence, merely in the condition of fibro-cartilage. Again, the ends of the bone may be rounded and smoothed off; they may be moulded so as to fit one against the other, and thus become connected by a sort of ligamentous substance surrounding them like a fibrous capsule. It has been said that bones do not unite readily in people advanced in life—that they do not unite readily in patients who have lost a great deal of blood, or who have had a long continuation of discharge, as from a compound fracture; and that they do not unite readily in persons in a state of pregnancy. But I think you will find them unite readily enough in time under all these circumstances, provided proper care is taken. Even in cases of compound fracture, with due attention and judicious management, you will find the cure completed after a no great length of time, and the union to be permanent and firm.

The Process of Union is sometimes interrupted,

very unaccountably, even in persons at the middle period of life, and apparently in perfectly good health, and where tolerable care has been taken to keep the parts in apposition. But I believe that this stoppage of the process of union arises generally in consequence of the bones being moved about too frequently, from time to time. In fact you may see some people treating fractures as if they had the intention of making false joints. They treat them just as they would treat animals in experimenting for the purpose of producing and examining false joints. They put the broken limb upon a soft pillow, allowing the muscles to disconnect the ends of the bone by every spasmodic action that may take place, and then they are adapting and reducing the fracture every two or three days, without adopting efficient means to retain it. When they come to look at the limb, perhaps the position of it does not please them, and they pull it out for the purpose of putting it into a better shape. You cannot be surprised in such cases if you find that the fracture does not become consolidated. Even in recent works you find a recommendation given to allow the broken limb, say when the thigh is the seat of the injury, to be in what is called a comfortable position (as if a patient's comfort and happiness could be enhanced by the constant grating and shifting of the rough extremities amongst the muscles, vessels, and nerves, in a state of high excitement and inflammatory action), and then at the end of six weeks you are to pull out and reduce the limb and put it in splints. This is the march of improvement in surgery with a vengeance. Why the greatest tyro amongst you must be aware that we are in the habit in this hospital of taking off our apparatus at that time, and looking upon the cure as all but complete. Patients themselves are sometimes to blame for the interruption of the process of union. I think I mentioned to you on a former occasion, that I never had but one case, the charge of which I had from the first, of the many that have come under my care, in which the bones did not unite; and that arose from the misconduct (if I may be allowed to say so) of the patient. It was in the person of a gentleman who had met with a fracture of the bones of the fore-arm, by being pushed down in the street. On the evening of the second day after the arm was put into splints, he cut them very short, in order that he might be at liberty to use his fingers in playing cards. I gave the case up. Some time afterwards he got upon

* In one of Mr. Cooper's patients in the hospital (a woman, fifty-six years of age), the bones gave way again at the original point of solution of continuity, on the seventy-second day, in consequence of her exerting herself to turn in bed.—REP. L.

horseback, and not being a very good performer in that line, he tumbled off, and in this second accident bent the arm backwards. It then became quite loose, and he had great difficulty in getting the extremity brought into a condition in which it afterwards could be at all serviceable to him.

Treatment of Disunited Fracture.—Setons.

The patient who was here lately with a disunited fracture of the bones of the forearm, received the injury nine months previous to his admission. He was admitted on the 4th of November last. He was caught between two wheels of a locomotive engine, by which he sustained a compound fracture of the left ulna and radius. He stated that the bones were broken into four pieces. It frequently happens that when a man ge's a limb broken, he wishes to make it appear that his sufferings have been very dreadful, that the case has been much worse than it really was, that the limb has been smashed into a great many little pieces. However, in this instance, both bones were fractured. I presume there was only a solution of continuity in each bone at one point. They were broken in two pieces, not in two places. The arm was put up in splints. He returned to his work after the accident too soon, and he found it again necessary to pay some attention to the limb. When he came here there was a perfect false joint; there was no osseous union whatever. The man's general health was very good, and the case seemed favourable for an attempt to bring about a union.

Now, you are no doubt perfectly well aware that various means have been employed for the purpose of bringing about union in the case of disunited fracture. The ends of the bone have been rubbed against each other so as to excite inflammatory action, which might be followed to a certain extent by a deposit of new matter. But a good deal of excited action must be produced, before osseous matter will be formed, or matter that will unite firmly and immoveably the two ends of a broken bone: that plan has completely failed again and again, and is not to be depended on more than long-continued pressure, which plan some time since had its advocates. It was also proposed by Mr. White, of Manchester, to cut down upon the fracture, and to take away a part of the ends of the broken bone. I should have stated that it has been said by Mr. Benjamin Bell, Mr. Wardrop, and others, that the existence of disunited fracture sometimes depended on a portion of muscle being interposed between the two broken ends; and they have talked of cutting down upon the fracture and taking away that portion of interposed muscle. Now that is certainly a very absurd notion. It is a notion which has never been, and which I really think is not likely to be, acted upon. But it has been proposed to cut down and take away the ends of the fractured bone. That operation has been performed, but it has failed also. It is a difficult and severe process, and even when it has been done, and done effectually too, a cure has not followed. I have never attempted this operation but in one instance, and that was in the case of a disunited fracture of the humerus. As you will see in my collection, I cut off good large portions of the end of the bone; but although I cut them off completely, and although the limb was carefully kept in splints for two or three months, no union ever took place. It has been proposed likewise to cut down upon the fractured bone, and to destroy the smooth substance which you find at the ends of the bone, with some sort of escharotic,—with the *lapis infernalis*, for instance,—but this has also failed. A proposal, I think, has been made to denude the ends of the bones, to drill holes in them, and to connect them by pieces of silver wire. It is too artificial a mode of proceeding, and I should fear that from the exposure and bruising of the ends necessary for accomplishing the object (for perforations cannot be made unless by denuding the bone freely, and holding it securely by some contrivance till the object is accomplished), that necrosis, instead of union, would be apt to follow. In short, all measures have failed in some cases in bringing about a cure.

The practice I pursued in the case of the man in this hospital, to which I have alluded, is that by seton. It is a practice which was first employed by Dr. Physick, of Philadelphia; and it has been tried in this country, in some instances with success, and in others with no success. Mr. Wardrop tried it, and although that case was supposed to be completely cured, yet I happened to see the patient some fifteen years afterwards with his limb dangling like a flail. There had been no osseous union whatever. This operation, however, has been more successful than any of the others; and I believe it would be pretty generally successful, if the surrounding soft parts could be a good deal torn up, if the periosteum could be a good deal excited, and if a seton were inserted, as in this case, to keep up the action for a certain period, and retained only for a very short period. Generally, the seton has been kept in for a great many weeks or even months, and until its influence has in a great measure failed,—has, in fact, had the contrary effect to that intended,—has weakened the deep-seated action. If you will only consider the subject, you may easily suppose that retaining the seton for a great length of time will rather counteract your object than otherwise. Retaining it in for a long time is attended with considerable discharge, which is unfavourable to the process of healing, and which puts the parts in the

situation of a compound fracture, which is by no means desirable. I would have you, then, cut down upon the fracture, as I did here, avoiding the nerves and vessels of importance. Cut the soft parts as little as possible. Introduce an instrument between the two ends of the broken bone, turn it well round, and then insert a seton, not caring that it should go exactly betwixt the ends of the bone, because I would rather pass it over the surface of the broken ends, and perhaps one on each side. Shake the limb a good deal about, and move the instrument rather roughly. The object is to excite a considerable degree of action; when that has been done, put the parts into as good a situation as possible for healing, and retain them so without the least possible motion. You have then attained all the good the seton can possibly bring about, and if you keep it in for a longer period, it will have the effect, as I have said, of weakening the powers of the part. I now show you the instruments I used. I cut down upon the fracture with a bistoury. A sort of perforator was then introduced, which I turned round rather roughly and perseveringly, so as to lay bare the extremities of the broken bones. I was in no very great hurry to complete the operation; I rather wished that the parts should be a good deal disturbed, and that anything intervening between the ends of the bones should be destroyed. This needle was then passed, by which I carried a large seton through the arm. In a few days a second, a larger and coarser seton, was introduced, smeared over with red precipitate powder, so as to produce more and more excited action. The operation was performed on the 5th; it was followed by a good deal of swelling, great pain in the part operated upon, and considerable fever. On the 11th the fresh seton was substituted for the other. On the 12th it was withdrawn entirely, and forthwith splints were applied. On the 27th of the same month, in consequence of great pain, one of the splints was removed, and a collection of matter evacuated. The splints were applied in such a manner as to allow the wounds to be exposed; one of them could be taken off without disturbing at all the position of the parts. The ends of the bones were kept in accurate apposition until the 19th of December, and on the 30th January the patient was discharged cured, and with the bones firmly united. I have succeeded in other instances, both in the upper and lower extremity, in bringing about union by the same process, as have many other surgeons, both in this country and in America. I may mention one in particular, my friend, Dr. Mott, of New York.

I have nothing to add to this statement, excepting that in a similar occurrence in any situation of the body, I should be very much disposed to follow the same practice. I should be inclined to try the seton; it is a less severe operation, and more readily accomplished,—accomplished with less risk to the patient than any other. I should take the seton away at an early period, because I should consider that by so doing I had a much greater chance of obtaining my object, than I should have by retaining it for any lengthened period.

Exuberant Callus.

Next Friday I shall go to the subject of tumours of the mouth, and some time or other I shall revert again to that of fractures. I shall then say something to you of the exuberant callus which is sometimes to be met with, and point out to you the necessity and means of avoiding such deformity. It is merely a clumsy excuse for having the bones placed one over the other. I shall illustrate these remarks by cases, preparations, and paintings. Here is a drawing explanatory of the process resorted to by nature to get rid of exuberant callus, made by the former clinical Professor of Surgery in Edinburgh. He is now-a-days not a very good draftsman, but he is a very good surgeon; and even now, I believe, though far advanced in life, as keen in acquiring information on all subjects, and as well informed as to the state of surgery, as younger and well-employed practitioners. You here see how the absorbents mould the parts, accommodate them to their awkward situation, and bring them into a more straight and handsome shape. I shall show specimens to you where ends of bones are so awkwardly situated, that they lie side by side for a great space; and where, nevertheless, strange to say, the cure is completed perhaps as rapidly as if the bones were laid end to end; but then, the patient in such cases is left with a very awkward extremity, perhaps shortened to a very great degree, and much distorted. It is an object of the greatest importance to bring a patient as much as possible, after such an accident, into the situation in which he was before the injury was sustained; and if you pursue the plan I have pointed out, you will be able to leave the extremity of the size and shape it was before. I do not think that in this hospital we have hitherto had a single case of deformity, or shortening of the limbs, following fracture. We have had very many bad ones to manage.

ON THE

STRUCTURE AND USES

OF THE

"THYMUS GLAND."

By WALTER DICK, *M.D., Glasgow.*

THE saying is at least as old as Galen, that there is no part without its use. As this is generally admitted, and as each succeeding discovery in physiology confirms it, we are thereby stimulated to investigate the *arcana* of the animal economy, however small the prospect of success. The uses of the spleen, and the thyroid and thymus glands, are problems on which the ingenuity and talents of physiologists have been long exercised, but hitherto no satisfactory solution of any of them has been advanced. Some plausible conjectures, it is true, have been entertained regarding the spleen; but any opinions concerning the uses of the thymus gland, which have yet been advanced, are not deserving of serious consideration. Anatomists now generally sum up its physiology, after having described its situation and structure, by simply saying, that it serves some unknown purpose during fœtal life. When we consider its size, we are naturally led to believe that it must minister to some important end. Before entering on the consideration of this subject, I shall premise a few remarks on the structure of the organ in question.

The name, thymus gland, which it has received, implies the belief that it is of a glandular nature, and as such it has been described by all those who have written about it. It is of moment to ascertain if this be really the case, for so long as it is called a gland, or supposed to be of a glandular structure, so long shall we be inclined to the supposition, that nature has formed it to separate from the general circulating mass some fluid of importance in the fœtal economy. What analogy is there, we may ask, between the thymus and any other gland, conglomerate or conglobate? Is it the granular and cellular appearance which a section of it presents, that leads to the belief of its being a gland? If so, it is an improper ground of belief. For we find the cellular tissue, in the soles of the feet and palms of the hands, presenting a similar appearance. I would not be understood to infer from this, that the thymus body is merely cellular membrane and fat. Sir Astley Cooper has given a pretty minute description of it; but his description, I am inclined to think, is somewhat biassed by his belief in the glandular nature of this body. If it were a gland, it would certainly have some excretory duct to eliminate its secretion. No one, however, has ever discovered such a duct, even after the most patient and careful examination. Hewson himself, who believed in the glandular structure and function of the thymus, fruitlessly sacrificed many animals in searching for it; and he was equally unsuccessful in his examinations in the human subject. Parity of structure does not imply parity of function. If it really possessed an excretory duct we should, *a priori*, expect, from the size of the organ itself, that it would not be exceedingly small; and when such men as Rysch, and Malpighi, and Mascagni, did not detect it, we may safely conclude that no such duct exists.

"Toutes les glandes ont des conduits," says Bichat, "destinés à rejeter au dehors le fluide qu' elles separent de la masse du sange;" or, "comme ils ne se trouvent que dans les glandes, on doit les considerer avec le tissu propre de ces organes." Since, then, all glands have excretory ducts, we must either admit that the thymus is a solitary exception to the general law above stated, or, that it does not perform the function of a gland. Whether it be more reasonable, or more consistent with facts, to come to the former than to the latter conclusion, I leave others to decide for themselves. I myself have decided in favour of the latter.

If I have proved to my own satisfaction the non-glandular nature of this body, I surely shall not be accused of presumption in proposing to change its name, from thymus gland, to thymus, simply. For, in anatomy, names are often bestowed upon parts from their supposed uses or structure; and when a name is bestowed erroneously, as I think that of thymus gland is, by continuing to use the name, we are apt, as Magendie says of another subject, "to give permanence to an opinion which is by no means proved."

The thymus is lodged in the anterior mediastinum. It is in contact anteriorly with the sternum, and descends from the lower part of the neck nearly to the diaphragm, being, in the full-grown fœtus, according to Boyer, "tres volumineux," and lying on the left subclavian vein, part of the trachea, and front of the pericardium. "Elle present une échancrure qui la divise en deux portions, ou cornes, dont la droite est ordinairement plus grosse et plus longue que la gauche. Il est rougeâtre et très mou dans le fœtus. Les lobules dont ce corps est compose sont creux, et leur cavites communiquent les unes avec les autres. Si l'on fait une œverture dans une partie quelconque du thymus il en sort une quantite de suc laiteux, souvent teint de sang, et coagulable par l'alcool." This description, so far as it goes, agrees with Sir Astley Cooper's; but Boyer makes no mention of what that distinguished surgeon calls the reservoir of the thymus.

The opinion I have formed as to its use,

leads me to think, that the larger it is in the fœtus, while of natural structure, the respiration will eventually be the more full and perfect. The thymus receives its blood chiefly from the inferior thyroid and mammary arteries, and returns it into the neighbouring venous trunks. Its nerves are delicate and difficult of demonstration, and come from the phrenic and great intercostal.

Numerous lymphatics have been seen by different anatomists ramifying over its surface. Some have even supposed that these were its excretory ducts. If the lymphatics are more numerous here than elsewhere (which I believe is not the case), it is probably because they have been more assiduously sought for. Indeed when men believed that the thymus was a gland, it was necessary for them to believe that its secretion must be removed from it somehow or another, and certainly under this impression it was natural enough for them to look upon the lymphatics as performing this office. But if we have shown that the thymus is not a gland, we cannot any longer view the lymphatics as its excretory ducts, and we need no longer wonder why an excretory duct has not been discovered belonging to it.

Boyer, who believed in the glandular structure of the thymus, assigns as the reason of its not, like the other glands, possessing an excretory duct, its being "une glande aveugle, d'où il ne sort rien lorsqu'on la presse sans l'avoir ouverte." It is difficult, however, to understand what he means by "une glande aveugle," and to me it appears equivalent to saying, that it is not glandular. For what is the characteristic of a conglomerate gland? Is it not its secerning power, and its possessing a duct to convey from it the fluid which it secretes? The thymus bears no resemblance to a conglobate gland; we, therefore, cannot agree with Boyer, when he says, "On peut donc, le considerer comme une glande conglobée ordinaire, mais d'une texture plus molle."

From what we have premised, I think we may conclude, that the thymus is not a gland, and resembles neither those belonging to the conglomerate nor conglobate class, but is an organ sui generis, and of a cellular structure, and, as dissection shows us, readily undergoes the process of absorption soon after birth.

We will now state some of the opinions that have been entertained respecting the uses of the thymus. Heister, when speaking of this subject, says, "Forte lympham secernit, eamque per vasa lymphatica ductui thoracico infundit, pro sanguinis et chyli dilutione, secuti glandulæ mesentericæ et pancreas agunt in diluendo chylo. Major huic usus est in non natis, quam in natis: quia illi respiratione adhuc destituantur, a quo tenuitas sanguinis maxime procuratur." The use here assigned to the thymus is, I think, neither probable, nor plausible,—improbable, because we have advanced sufficient to render at least questionable, the glandular nature of the body,—not plausible, because the fluid it is supposed to secrete must come from that which it is intended to dilute; and, consequently, instead of diluting it would rather tend to inspissate the blood.

Bellingeri supposed that the thymus was formed to prepare during fœtal life a liquid, for the nourishment of the child, which he supposed was carried to its mouth by several ducts, which of course his fancy rather depicted, than his eyes ever saw. It appears to me, indeed, impossible for one organ or system of organs, to render the blood of the mother more nourishing for the child in utero; and consequently the use just mentioned appears to me, to say the least of it, a work of supererogation. We were astonished to find Sir Astley Cooper entertaining this opinion. He says,—"As the thymus secretes all the parts of the blood, viz., albumen, fibrin, and particles, is it not probable that the gland is designed to prepare a fluid, well fitted for fœtal growth and nourishment, from the blood of the mother before the birth of the fœtus, and consequently before chyle is formed from food; and this continues for a short time after birth, the quantity of fluid secreted from the thymus gradually declining, as that of chylification becomes perfectly established?" Hewson supposed that the thymus co-operated with the spleen in the production of the red globules of the blood. Surely this also is a work of supererogation. For do not the globules circulating in the vessels of the fœtus come directly from the blood of the mother? Every view, then, which has been taken of the functions of the thymus, is, in my opinion, unsatisfactory and incorrect; and the almost universal opinion now is, that its use or uses, whatever they may be, are unknown. "Les usages du thymus," says M. Boyer, "ne sont point connu, mais son utilité quelle qu'elle soit est plus grand dans le fœtus que dans les adultes."

I shall attempt to show that it is formed in the fœtus, to be of more service *after* birth than before it.

Anatomists and others are in the habit of speaking of the cavities of the chest and abdomen, which, as Mr. J. Bell shows, is, strictly speaking, erroneous. There is no vacuity in either of those regions, every point being accurately filled up by its contained viscera, and no cavity or empty space existing, till formed by disease or accident. Some may think that in the child *in utero*, there are parts which might have been left with advantage unoccupied. One of these, perhaps, would be the front of the chest. But the child being subject to the influence of atmospheric pressure, this could not be the case, and consequently that part must have been filled up by the lung, or, as nature has

done, by the thymus. It appears to me that if the lungs had occupied, during *intra-uterine* life, the place of the thymus, there would be too little room for the expansion of the lungs, considering the density of their texture before they have been permeated by air; in fact, there would have been a useless expenditure of lung,—a circumstance we cannot expect to be effected by nature, who is ever perfect in her operations.

When the child is separated from the mother, respiration is established, to enable it to maintain an independent state of existence. Nature seems anxious to have this function as perfect as possible, by diminishing the size of the chest of the child in *utero*, by the position of the child, and the great size of the thymus and the liver.

We have already spoken of the size of the thymus in the full grown fœtus. The liver at that period is very large, receiving a great supply of blood from the umbilical vein; but after birth, this supply being cut off, it very soon begins to diminish in bulk. It is evident that as both the thymus and liver decrease, the capacity of the thorax will be increased. I know that Tiedemann and Gmelin have made it appear probable that the liver of the fœtus is large, and receives a large supply of blood, in order that the blood may undergo a process of decarbonization. This may be, but I think nature also intended it to co-operate with the thymus, in rendering respiration more perfect, by shrinking after birth.

In taking a glance at man in infancy, childhood, and adult age, we see, in the first of these periods, a tender, feeble creature, incapable of making almost any exertion of his voluntary muscles, with a quick respiration and hurried pulse. We see him again in childhood, not quite so delicate, exercising with tolerable freedom his locomotive organs, yet incapable of any great or prolonged physical effort. His respiration not so rapid and pulse less quick. When he attains adult age we find him a very different creature. We see him now performing great bodily efforts, and that for a considerable time. His respiration and pulse are far below, in point of frequency, what they formerly were. In the first of these periods of life we find the size of the chest considerably encroached upon, and the free play of the lungs interfered with, by the thymus and liver, and consequently the respiration is hurried. In process of time the thymus all but disappears, and the liver gradually attains that size which nature has assigned it. With these changes the respiration becomes slower, and of course more perfect.

A little reflection will show us that the extent of the pulmonary tissue is less, and that less air (comparatively speaking) is inhaled into the lungs of a very young child than in more advanced age. A little further reflection will perhaps convince us that this results from the presence of the thymus. As the subject grows older, his movements become more extensive and frequent, and consequently there is more necessity for a free supply of air, and how could this have been afforded better than by the mechanism which I think nature has employed, viz., by forming the thymus large in the early period of life, so that by its shrinking, when necessity requires it, a greater surface of lung may be afforded for the permeation of air and purification of the blood?

We know that when the thymus, in place of becoming absorbed, continues through life, or becomes diseased, it gives rise to dyspnœa.

It may be asked, What is the cause of this body shrinking or becoming absorbed? We might anwer by asking, How does the ductus arteriosus, or the foramen ovale of the heart, become obliterated? We understand *why* they become so, but we are ignorant of the *how*. The absorption of the thymus, however, results chiefly, I think, from the pressure of the lungs against it during respiration. Boyer says, "Cette compression devient tres manifeste, lorqu'one pousse de l'air dans les poumons."

Glasgow, April 1836.

MILKY APPEARANCE OF THE BLOOD.

Antoine Gorla, of Cremona, a dealer in lemonade, aged 47, stout, and muscular, and addicted to wine, became indisposed the next day after a debauch. He was received into the hospital of the town on the 22nd of May, and showed the following symptoms:—Violent fever; pulse hard and intermitting; constrained respiration, without cough, expectoration, or pain of the region of the heart. He was unable to lie either upon the back or the sides: tumid abdomen; costiveness; scarcity of urine; the inferior extremities œdematous; general prostration of strength. The whole skin was pale, particularly that of the face. Blood-letting was prescribed, and a dose of *oleum ricini* was given.

The next day, viz. the 23rd, the blood appeared as white as milk, and the crassamentum was as purely white as the serum. These two substances could not be distinguished from each other, except by their consistence and fluidity. It is necessary to remark that the blood did not exhale the odour of milk. This blood was reserved for analysis. The same symptoms presented themselves in the patient to day as yesterday, generally speaking, except that there was more debility of the system. He was again bled, and a dozen of leeches were ap-

plied to the epigastrium. A dose of *oleum ricini* was ordered, and an emollient lavement, morning and evening. It was afterwards found that the blood removed by venesection was equally white as that of the first bleeding, and the same phenomenon presented itself with respect to the blood removed by the leeches.

On the 24th, the symptoms were nearly the same, and the blood taken from the vein yesterday, presented the same milky appearance. He was bled to-day again. A solution of manna was prescribed, and leeches were applied in the evening.

25. He has had some sleep since yesterday, and the respiration was more free. The cough less painful, and less pain of the side. In a word, he was materially relieved. He was again bled, and the emulsion was continued. The blood of yesterday, and that of to-day, was still white. The solution of manna was continued.

26th. Tranquil sleep during the night. The respiration was normal, but the fever was still intense. In all other respects, however, he was materially improved. He was bled again to-day, and, upon cooling, the blood separated into two portions. The one was white, and had none of the constituents of healthy serum. The other formed a cake composed of globules and of fibrine. The cake was covered with a shining white rind.

On the 27th a marked amelioration was observed; the secretions and the excretions were duly performed, and the appetite was improved. He was again bled, and had a blister applied to his side. The blood was now natural in its aspect. The patient soon afterwards became convalescent, and left the hospital in a few days, being restored to health.

The results of an analysis of the blood (taken at the first bleeding) by M. Le Canu, and of which we do not think it needful to give the details, are as follows. One thousand parts of this blood contains: —

Water	794
Oleine, margarine, stearine ..	117
Acidulous soap, cholesterine..	108
Albumen	65
Salts and animal extractive	25
	1109

The above comprises the sum and substance of a very interesting case, to be found in a late number of the *Archives Générales de Médecine*, by Dr. Giovanni Luigi Zacarelli. It is somewhat strange that although, in a quantitive manner, 1000 parts are mentioned, the constituents, or proportionals, make 1109; nor do we find any *errata* given to set this right in any subsequent number of the journal. Oversights may and often do occur with the most accomplished chemists; but as the results are given consecutively, and in line, without being totalled, we infer that this serious error has arisen through that deficiency. Continental writers have long been most careless in passing their figures, proper names, and quotations, through the press. Latterly they have somewhat improved, but have yet much to mend. It is impossible in periodical publications, where the press, like the tide, will wait longer than a certain time for no man, to be perfectly exact, and the immense mass of writing that passes under the journalist's eye, comes to be viewed sometimes almost mechanically; but in figures an extra degree of care is demanded, because the reader is in most instances enabled himself to correct all other errors. Yet in the former item are our foreign brethren least precise. But to return to the case under consideration.

It is to be regretted that the quantities of blood taken at the different bleedings of the patient, Antoine Gorla, are not set forth. This is a serious omission in a pathological point of view. The treatment was, with the exception of the venesection (as is usual with the French physicians), very simple, or even inert. At the same time we must acknowledge, that from this circumstance we are warranted in attributing the cure solely to the removal of the diseased blood from the system. This case may be classed with those presented to us by Dr. Wardrop in his lately published valuable Treatise on Blood-letting. Every addition to our knowledge of the vital fluid will prove to be of high importance to medical science in a practical point of view. There is abundant room in the field for investigators both of normal and diseased blood, and we beg for additional opportunities of adding to the mass of valuable information on the subject, which has been communicated to the profession in these pages, particularly during the last half-dozen years.

FRENCH JOURNAL.

Archives Générales de Médecine. January February, March, 1836. Paris.

The first three Numbers of this journal for the current year contain the following original articles:—

1st. On Diseases of the Lymphatic System; Part 2. By M. Velpeau.

2nd. On the Extirpation of a Goitre, by M. Roux; with some remarks on this affection, by M. Rufz.

3rd. Practical Observations on Incontinence of Urine. By M. Mondiere.

4th. On the Anatomical conditions of Diseased Bones. By Professor Gerdy.

5th. Analytic Researches on some points in the History of Leucorrhœa. By M. Marc D'Espine, ex-interne of the Venereal Hospital.

6th. The Consecutive Accidents of Fracture. By M. Guyot.

7th. Clinical Observations on the Diseases of Old Persons; Part 3. By MM. Hourman and Dechambre.

8th. On the presence of Pus in the Lymphatic Vessels of the Uterus after Labour; Part 2. By M. Duplay.

2. EXTIRPATION OF A GOITRE.

The following is a brief history of the case operated on by M. Roux:—

A man, twenty-two years of age, of a lymphatic constitution, whose grandmother and brother were affected with goitre, presented a few symptoms of scrofula at the age of ten years; these were soon removed, but an enlargement of the thyroid gland, which appeared soon afterwards, persisted, and from that time continued gradually to acquire an increased volume. At the time of his reception into the *Hotel Dieu*, the tumour was as large as a large apple, extending from an inch below the chin to the sternum, towards which it had been carried by its weight, for it was easily raised up from that bone. Although the tumour did not produce any severe accident, M. Roux consented to perform one of those operations de complaisance to which he is too much inclined. The enlarged gland was accordingly removed in three different portions. About a pound and a half of blood was lost; from forty to forty-seven ligatures were applied on the arteries and veins, chiefly the latter. For the first few hours the patient seemed to be going on well; however, he died suddenly, after a lapse of fifty-six hours, with some symptoms of difficult respiration, and after death, nothing was found but some slight traces of lobular pneumonia.

Undoubtedly, the most important part of the case just noticed, is the operation, and the question to which it gives rise, How far is the operation justifiable in cases of goitre? Of what advantage is it?

The number of these operations recorded in the annals of the science are not many; the greater part seem to have been performed in Germany. Hedenus says, that his father extirpated the tumour six times with success, but he does not cite an example; he also mentions three operations practised by Graeffe, and gives two of them in detail. Gooch mentions two cases operated on in England, one only with success; the second died from hemorrhage. In France we find several attempts made at removing goitre. Desault operated twice, once successfully. M. Brun, in his thesis, speaks of a case which he saw operated upon by a country surgeon, which terminated fatally on the spot, by hemorrhage. M. Ruflier mentions a similar occurrence in his presence. Finally, the late Baron Dupuytren twice attempted a similar operation, and lost both his patients.

The greater part of these cases are reported in so imperfect a manner, that they cannot serve to throw much light on the history of the operation. There are, indeed, only four that we can consider as complete. Desault's case, given by Giraud (see Cooper's Dictionary, Art. "Thyroid Gland"); Dupuytren's, by M. Rullier (*Thesis*, 1808); that of Graeffe, by Hedenus; and M. Rufz's report of Professor Roux's case, which we have now noticed.

Let us briefly notice a few points in these four cases. Two of them were mortal and two successful. In the two successful cases, those of Desault and Graeffe, only one lobe of the gland was extirpated.* In Dupuytren's case, the whole gland was removed; in that given by M. Rufz, M. Roux contented himself with extirpating the right lobe. We do not find hemorrhage occurring as an accident in any of these four cases; the precautions taken in all were the same, viz., ligature of the arteries and veins before or after their excision.

The method of operating was nearly the same in the four cases; Desault and M. Roux employed the T incision; Dupuytren a crucial [illegible]; Graeffe an elliptical incision. The tumour removed by Dupuytren was undoubtedly the most voluminous, being as

* We may here note, *en passant*, an error into which Professor Cooper has fallen, who describes Desault's case as one of extirpation of the thyroid gland.

large as a young child's head. That operated on by Desault seems about the same size as M. Roux's. In Graeffe's cases the swelling was much smaller. M. Dupuytren's case terminated fatally in thirty-six hours, that of M. Roux in fifty-six, both patients having presented, near the fatal period, great difficulty of respiration and paleness of the face.

The facts just noticed are too few to warrant a decisive opinion on the applicability of this operation. However, when we reflect that the goitre is rarely susceptible of assuming the cancerous degeneration, and is very seldom productive of accidents endangering the patient's life, we are inclined to consider it, with M. Rufz, as one of those operations of complaisance which a prudent surgeon will seldom or never attempt.

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3. PRACTICAL OBSERVATIONS ON INCONTINENCE OF URINE.

Nocturnal incontinence of urine is an affection frequently met with in children, and sometimes persisting after puberty; and if the physician be rarely consulted for this inconvenience, it is in consequence of the opinion almost universally received, that medicine has no power in alleviating it.

Nothing is more incomplete than the account given of this affection even by our standard authors. Underwood speaks of it in his usual rambling, and almost unintelligible, way, without endeavouring to distinguish any of the different species, confounding essential with symptomatic incontinence, and confining himself, as is his custom, to a simple enumeration of remedies, without condescending to inform us under what circumstances such or such a remedy should be administered,—when the stimulating system is advantageous, or when the tonic. His commentator, Dr. Hall, a physician of the modern school, seems little content with the vague generalities of his author, and endeavours to ascend to the causes of nocturnal incontinence, which he attributes to derangement of the digestive organs, and considers as almost exclusively confined to boys. The latter idea is certainly erroneous. We have seen several cases in young girls; we have heard of others; and we could point out a great number in the records of modern periodical literature. We might also remark, that Dr. Hall has fallen into the same vagueness of expression which is so common in Underwood's work. What is the meaning of "derangement of the digestive organs?" How many different kinds of derangements, organic and functional, are the digestive organs subject to?

But enough of this little digression, into which we have been led, by having consulted the last edition of Underwood, p. 388, on "incontinence of urine." The remedies commonly employed in England against this inconvenience are enumerated at sufficient length by Underwood, in the article just mentioned. The same uncertainty seems to reign in France. Tonics have been frequently given without any success. Electricity has equally failed. M. Canin (*Arch. Gen.*, first series, t. 12, p. 618) mentions two cases treated successfully with dry cupping to the perineum. Dupuytren employed cold baths with a good deal of success. A case is reported (*Nouv. Bib. Med.*, t. 2. p. 121. 1828) in the person of a young girl, twelve years of age, where this bad habit was cured by plunging her into a cold-bath, and withdrawing her immediately. The incontinence gave way to the first application, but the immersions were continued, through precaution, for a few days. M. Lallemand, of Montpellier, seems to have obtained equally beneficial results from the use of aromatic baths. Underwood recommends blistering and cantharides as the principal remedies on which we should depend in this affection. They are certainly beneficial in many cases, although M. Guersent says he never obtained any advantage from employing them. M. Leiger (*Roux's Journal*, t. 4, p. 72) has published three observations of this affection cured by the internal use of cantharides; two of these refer to two *girls*, sisters, one of fifteen, the other thirteen years of age.

Finally, the author of the memoir now before us proposes the extract of nux vomica as a very efficacious remedy. The following observation (with some others reported by the author) tends to support this opinion:—

A young *girl*, twenty years of age, consulted M. Mondiere for an incontinence of urine, with which she had been affected from the age of six years; her mother, during a long period, continued to watch and awaken her in the night, but this did not prevent her from sometimes wetting the bed towards morning. No remedy was employed

at this period, nor before the age of twenty, when the author recommended the use of *Extract. Nuc. Vom.* gr. viii; *Oxy. Ferri Nig.* 3i. Fiant pill. 24; quarum ter in diem.

The patient had taken only twelve pills when the incontinence of urine had ceased completely, and did not return, although the patient often went to bed without having emptied the bladder. To prevent any danger of relapse, the extract was administered as far as twenty-four grains.

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5. SOME POINTS IN THE HISTORY OF LEUCORRHŒA.

This is a conscientious memoir by a pupil of M. Louis's school (M. Marc-D'Espine), founded on researches conducted during a year of dresserships at the Venereal Hospital, and presenting an especial degree of interest from the positive manner in which they were made. In fact, what patients, except the inhabitants of the Hopital du Midi, would submit to daily examination with the speculum? Yet it is from the opportunities he possessed of having recourse to this method of investigation, that the researches of M. Marc-D'Espine acquire their peculiar degree of importance.

The author commences by explaining the term leucorrhœa, which he defines "a chronic discharge of colourless fluid occurring in females otherwise healthy, and free from all other disease of the genito-urinary organs," and then demonstrates the necessity of not implicitly relying (as most authors have done) on the accounts given by the female, but having recourse to an immediate examination of the parts. The influence of certain general conditions on leucorrhœa and its various forms, is the examined.

Climate.—The women placed under the care of the author were all inhabitants of the north and centre of France. Amongst 80 of these, 27 were affected with leucorrhœa for a greater or less period: 27 had never had a discharge of this kind. The observations of M. Girard were made on inhabitants of the centre and south of France: of 25 women, 7 were affected with whites; 18 never had any trace of the disease. Hence one third of the women in the north were found exempt from leucorrhœa, while nearly three-fourths of those inhabiting the south were unaffected, a fact which seems to prove evidently the influence of *climate*. Amongst 53 Parisians subject to leucorrhœa, 26 had been affected before, 18 during, and 9 a long time after, the period of the first menstruation: thus the early commencement of leucorrhœa may be regarded as a proof of its connection with constitutional disorder; especially in the inhabitants of a large city.

Temperament.—It is not easy to characterize the different temperaments, because authors are not agreed on the physical signs peculiar to each; but are we entitled to say with most writers, that leucorrhœa is a constitutional condition of the lymphatic temperament? Has this been proved? In 47 cases which M. Marc-D'Espine analyzes, he finds that in the north and centre of France women with dark or chesnut-coloured hair, and brown or black eyes, are more subject to this disease than women with blue eyes and light or chesnut hair.

Constitution.—The appreciation of this cause is approximative; the author distinguishes the strong, middling, and weak constitutions. Of 19 women never subject to leucorrhœa, 4 were robust, 7 of a middling constitution, and 8 of a weakly one. Of 19 other women more or less subject to habitual leucorrhœa, 6 were robust, 9 of a moderately good constitution, and 4 weakly. A comparison of the above results would seem to prove that women of good constitution are more subject to this discharge than those of a feeble one. Again, amongst 20 females affected from infancy, 7 were robust, 7 moderately so, and only 6 of a weak constitution.

Results furnished by Exploration with the Speculum, to determine the Seat and Nature of Leucorrhœa.—When we examine an adult and quite healthy woman with the speculum, we commonly find the vagina lined with a slight humidity, and the os tincæ closed by a drop of fluid which is sometimes transparent, sometimes opaque, or striated with white and yellow lines like membranes. In some cases the natural secretion of the uterus, instead of being glairy and consistent, is altogether watery, and it is difficult to ascertain if the quantity be greater than it should be. Finally, the author has sometimes found the os tincæ quite dry, without any appearance of uterine secretion in the vagina. What has just been said refers exclusively to secretions coming from the uterus; they form the most important point, as constituting the greater part of the dis-

ease called leucorrhœa or whites. There are, however, secretions peculiar to the vagina. Sometimes the vagina was covered with a matter analogous to pus, and when this coincided with uniform redness of the mucous membrane, it probably depended on blenorrhagic inflammation. In some other cases the vagina was lined with a creamy-looking secretion, although its sensibility and colour were natural, and the discharge from the uterus altogether normal. The abundance and nature of the uterine secretion are points which M. Marc-D'Espine has studied with a great deal of care.

In 193 females affected with leucorrhœa and examined by the speculum, he found

In 23 cases, the os uteri and vagina free from all trace of anomalous secretion.

In 40 cases, the uterine discharge was manifested by a simple drop hanging from the os tincæ.

In the remaining 190 cases, the uterine discharge was much more abundant.

Let us make a few observations on each of these categories. In the 23 cases where no trace of discharge could be discovered, 10 coincided with suppression of the menses, for two, three, or more months.

In the 40 cases of the second category, there was suppression of the menses eleven times; of the latter it depended in one case on pregnancy.

Finally, in the 130 cases of the third category, the menses were in retard 28 times; and in 10 of these 28, the women were pregnant.

From the above numbers we see that the secretion of the leucorrhœal discharge *from the uterus*, is not necessarily suspended during pregnancy, but almost always continues, while on the contrary it was suspended in a large proportion of cases of amenorrhœa.

Our space will not permit us to follow the author through his calculations on the influence of the condition of the os uteri on the nature of the discharge. From a comparison of 193 cases in which the speculum was applied, he shows that the watery and transparent albuminous discharge occurs in a majority of cases where the orifice of the neck is healthy; in the moiety of cases, where it is surrounded by a red circle, and only in a quarter where the os tincæ is ulcerated or much injected. It moreover would appear probable that the watery and transparent albuminous secretions belong properly to leucorrhœa, while the striæ with which it is mixed in the pus, depend rather on the effects of inflammation. It now remains to study the alterations of the mucous membrane of the vagina most frequently observed in leucorrhœa, their degree of frequency, and their influence on the nature of the secretion.

It is necessary to premise that the author quotes only those observations in which he was enabled to distinguish the discharge belonging to the vagina from that of the uterus itself. The alteration which presented itself much most frequently was a change of colour. In 80 cases, of which the author has preserved notes, there were scarcely two which exhibited any other alteration than a change of colour.

The facts observed (90) may be arranged in the following decimal table:—

	Mac. mem. pale.	Rose.	Uniform red.	Pointed red.
Without discharge from the vagina	0.585 —	0.404 —	0.411 —	0.000
White creamy discharge.....	0.268 —	0.355 —	0.363 —	0.500
Cheesy discharge	0.025 —	0.069 —	0.062 —	0.000
Puriform discharge.....	0.122 —	0.172 —	0.164 —	0.500

It follows from the above table that all the forms of vaginal discharge present themselves with the more frequency in proportion as the mucous membrane deviates from its normal conditions; and that, on the contrary, the absence of vaginal leucorrhœa is more frequent in proportion as the membrane of the vagina is healthy.

However, in a certain number of cases, we meet with every species of discharge, even when the mucous tissue is sound, and vice versa.

We also obtain from the above table a principal fact, viz.:—That every variety of secretion, even its complete absence, may coincide with all the various conditions of the vagina and os uteri; nevertheless, this does not prevent the existence of certain relations between such and such forms of discharge, and such and such lesion; exceptions occur here, as in every other part of medicine. The absence of discharge from the vagina or from the uterus is more frequent as either of those parts approaches its normal condition. The watery discharge, which appears to belong to the uterine cavity, is the form that seems next allied t a complete absence of discharge.

The above considerations are not, perhaps, all of them novel; but they deserve attention as being the first deduced from an actual examination of the parts in a large number of cases. As yet the history of leucorrhœa has been rather conjectural than positive.

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7. CLINICAL RESEARCHES INTO THE DISEASES OF OLD PERSONS.

This paper, by MM. HOURMANN and DECHAMBRE, forms Part 3 of a series of which we have already given a very ample account in previous numbers of THE LANCET. We now resume our analysis:—

Inflammation of the pulmonary tissue is undoubtedly the most dangerous, and, at the same time, frequent affection, observed in old persons. Though often concealed by the mildness of the general symptoms, or the prostration of an *adynamic fever* accompanying it, it is distinguishable from the pneumonia of adults, by local signs depending on the peculiar organization of the respiratory apparatus, and by anatomical characters of a kind proper to it.

The pulmonary tissue of old persons being the seat of a constant fluxion, to which we shall presently recur, presents a more or less marked degree of vascular injection, independently of all inflammation, and it would always be difficult to establish in the dead body the commencement and even progress of pneumonia, if we did not take care to observe with the greatest attention all the commemorative circumstances of the disease. The lung in aged persons may present the following degrees of sanguineous congestion.

1st. *Simple Engouement.*—Here the redness is sufficiently intense, but the lung is crepitant, and floats in water; when incised, a sanguineous foamy serosity escapes in abundance. The interlobular septa are still distinct. In a more advanced degree of congestion, we merely remark a deeper coloration of the parenchyma; the other characters remain the same. In an extreme degree of engouement the lung is livid, and the lobules almost confounded together. The parenchyma is more voluminous, and its cohesion often diminished; a dark liquid flows out after each cut, and we distinguish no crepitation, except under strong pressure. The pulmonary tissue does not float so lightly as in the former cases.

Desiccation of thin slices of the pulmonary tissue evidently demonstrates that the cells still exist, but they are considerably diminished in proportion to the intensity of vascular injection.

2nd. *Sanguineous Congestion, with Impermeability of the Pulmonary Tissue.*—In this form the lung presents itself under two aspects, quite distinct. In one, the compact parenchyma offers, when cut or torn, no trace whatever of the granular organization, considered as the anatomical peculiarity of the pulmonary tissue; in the other, these granulations are extremely well marked.

(*First Form.*) Here the tissue, varying from a deep blue to a dark or black tint, presents a cut surface perfectly smooth and homogeneous. The lobular septa are effaced; no crepitation; the tissue is either elastic, or, on the contrary, soft and destructible; when divided, no blood is discharged, but a fluid or viscous serosity, limpid or of a reddish colour.

(*Second Form, Red Hepatization.*) The red granulations are of two kinds; in one case they are regular and very voluminous, much larger than those observed in the pneumonia of adults. In the other case, though prominent, the granules are irregular, and seem to have a tendency to become confluent. In a majority of cases, the friability accompanying the red hepatization in old persons is far from equalling that observed in the pneumony of adults. The tissue is even very resistant, and is easily divided into thin slices; but it is less dense than an equal portion of hepatized lung in the adult, and seldom descends quite to the bottom of the water; in some cases it floats nearly at the surface. Perhaps this character may be explained by the naturally specific lightness of the lung in old persons. (See Part 1.) When the hepatized lung is divided, it constantly gives issue to a quantity of serum, free from air, and often but slightly tinted red.

3rd. *Suppuration of the Lung.*—Sometimes the suppurating lung, like one simply congested, presents a clear polished cut; in other cases the suppuration is granular (*gray hepatization*). This latter form is much more frequent, and is distinguished from the third degree of pneumonia in the adult by the volume of the granulations. The reduction of the gray granules may sometimes be extreme; they may be absent altogether, and we then often find in their place small cavities filled with pus, true abscesses in their commencement. However, extensive abscess of the lung is extremely rare, and during two years, in a great number of autopsies, the authors have observed only two examples.

There is a third variety of gray granulations which MM. Hourmann and Dechambre consider as belonging exclusively to old persons, and not described by preceding writers. In the only case of this kind they had occasion to observe, the lung was much increased in volume and density, and at the same time extremely flexible. The pulmonary lobules were transformed into globular granulations of a very large size, forming clusters separated by bands common to the neighbouring ones. Numbers of these grains

were red; others not quite gray, but of a fine yellow colour, like pale fat: they bore strong pressure without giving way, and the fine bronchial tubes at the same time discharged a matter of a mixed appearance, between true pus and expectoration. In proportion as this matter came away, the portion of lung became gradually less compact, but more elastic, and the yellow granulations evidently lost a part of their volume.

Two portions of the same lung, one after pressure had been exercised, the other as extracted from the dead body, were submitted to desiccation. The latter presented nothing more than the opaque plates of an hepatized lung. On the contrary, the lamellæ of the former exhibited a multitude of small irregular holes, which we must consider as so many traces of the pulmonary cells incompletely restored.

Besides the circumstances just mentioned there are several other peculiarities to point out.

1st. The diseased lung, especially if it belongs to the first type, commonly acquires a considerable volume: but in a lung of the second type (see 1st Part), retracted against the vertebral column, and presenting no marks of injection beyond the usual cadaveric engouement, the pulmonary tissue, when divided, has often been found almost completely filled with gray and red granulations.

2nd. Congestion, whether purulent or sanguineous, commonly occupies a great extent in old females, and it seems extraordinary how life can persist when so large a part of the respiratory tissue has ceased functioning almost instantaneously. In one case the authors found the right lung and the inferior lobe of the left lung in a state of gray hepatization throughout their whole extent, while the upper left lobe was gorged with blood and frothy matter.

3rd. The congestion very seldom assumes the lobular form, and the *vesicular* is still more rare.

Having thus described in a general manner the organic lesions observed in the pneumonia of aged persons, our authors compare them with the varieties of organization already described in their first memoir, and endeavour to explain the peculiarities of one by the peculiarities of the other. Thus, for example, having observed the absence of granulations in several patients who presented during life all the symptoms of pneumonia, they ask whether this peculiar disposition may not be accounted for by the extreme rarefication of the pulmonary tissue in the third type. The following is a resumé of their researches on this point.

They have found all the varieties of pulmonary rarefication (see 1st Part) presenting that condition in which the congested lung becomes impermeable, with or without friability, and in which, at a more advanced stage, the pus may be forced from one point to another, until arrested by the interlobular septa.

The condition in which non-granular impermeability is followed by a deposit of a demi-concrete matter in large cells, has never been seen by them except in lungs of the third type.

Finally, they have never discovered red or gray granulations in this latter variety; they seem exclusively confined to the first and second types. What are the consequences deducible from the above facts?

1st. The pus diffused and capable of being pressed by the nail, is evidently seated outside the minute lobular bronchi. Were not this the case, instead of passing along the interior of the parenchyma, the pus would issue in small drops from the incised surface; and far from being arrested by the interlobular septa, it would be driven by pressure into the larger tubes. It is in the tissue which Laennec has named *intervesicular*, that we find the pus in the form of suppuration now alluded to, and analogy would lead us to conclude the same for the congestion of blood which precedes the formation of pus; this opinion is still further supported by the results of desiccation, which shows us the cells conserved as in simple engouement, the slight injection of their parietes, and, on the contrary, the deep injection of the vessels ramifying in the intervals by which they are separated.

2nd. On the other hand the influence exercised by the changing form of the pulmonary cells on the form and even production of the granulations, seems to demonstrate that the latter are deposited either in the cavity or in the walls of these cells themselves. They are globular or irregular, according as the cells they occupy present these forms, and it is easily understood why they are never found in lungs of the third type. Compression and desiccation prove the truth of this. They never change the appearance of a granular lung; they never restore the cells either in cases of red hepatization, or in the gray hepatization with friability, a fact which proves that the granulations occupy the seat of the cells.

Finally, the manner in which the gray non-friable granules submitted to compression or desiccation sometimes disappear, aids us in determining their seat. We press a granulation, and there escapes, in little drops, from the neighbouring bronchial tubes, a matter of the same colour as the granule, which now disappears. Thus the observations of MM. Hourmann and Dechambre seem to demonstrate that the reduced gray granulations really occupy the pulmonary vesicles, and not the intervesicular tissue. This was supposed by M. Andral, who, when explaining the mucous and crepitating râles, conjectured that an

accumulation of sanguineous mucosity, or pus, in the vesicles, gave rise to the red and gray granulations, but adds, "These ideas are merely supported by probabilities; to obtain a certainty we should be able to demonstrate the inflammation of the vesicles, the scalpel in hand; but this has not yet been done, and perhaps will never be possible."

But are we to conclude, that all kinds of pulmonary granulations are produced by an accumulation in the vesicles of concrete blood in one case, and of pus or muco-purulent fluid in the other? The authors answer in the negative; for, although in the latter case, the fluid may be discharged by the bronchi, the volume of red granulations cannot be altered in the same circumstances; and if, as has been imagined, they were formed by a simple drop of coloured mucosity, imprisoned in the vesicle, this mucus ought to be discharged with still greater facility than pus.

The following is their description of the manner in which a pulmonary granulation is formed:—First, simple injection of the little membrane described in the first part as circumscribing the cell; secretion of a sanguineous mucosity into its cavity. Here the lung is still permeable. At a later period, penetration by the blood, turgescence of the parietes of the cell, and obliteration of its cavity. Finally, suppuration sets in, the granulation gives way, the pus occupies its centre, and transforms it into a small abscess.

In a number of succeeding sections, the authors enter into a minute description of the different forms of congestion in the lungs of aged persons; their friability and impermeability; from which they establish the existence of two species of pneumonia, with the anatomical characters distinguishing them; the first, constituted by sanguineous congestion, giving a smooth cut (*plasiform*), followed at first by impermeability, and then by a secretion of pus, has its seat outside the bronchial tubes, in the laminar tissue which separates them. The second species, on the contrary, occupies the tubes themselves, and depends on a granular engorgement of their parietes, or on the deposit of puriform matter in their cavity. When their cavities are too irregular, the granulations cannot develop themselves, but still the state of the lung evidently belongs to the second form. Guided by the analogy between the anatomical peculiarities of these two species of pneumonia, and the two forms of emphysema, the authors propose to distinguish them into *intervesicular* and *vesicular*.

We have next to examine the degree of frequency of pneumonia in old persons, its seat in the lung, the state of the bronchi, &c.

Frequency.—In 636 observations, there were found 110 cases in which a simple cadaveric congestion existed, without any sign of pneumonia during life. In 370 cases the patient recovered after having presented the stethoscopic signs of congestion; but here it generally occurred after long confinement to the lying posture, and under circumstances where its inflammatory nature might be sometimes contested. In 53 cases the patients all quite recovered, after having presented symptoms of pneumonia of the clearest kind. In 15 cases the patients died after presenting the same evident symptoms, but the examination of the bodies was not permitted. Finally, in 88 cases, there were found the anatomical characters of inflammation.

In the 88 latter cases 18 only belonged t the *non-granular* or *inter-vesicular* form, and of these 5 only with purulent infiltration. The remaining 70 presented the granular engorgement in its most perfect degree; hence the *vesicular* pneumonia bears to the *inter-vesicular* form the proportion of nearly 4 to 1.

Seat of the Inflammation in the Lungs.

1st. *Side affected* in the 13 cases, without suppuration; the engorgement existed 4 times on the right side, 3 times on the left, 6 times on both sides; in the 5 cases with effusion of pus, 4 times on both sides, 1 only on the right side.

2nd. In the 70 cases of vesicular pneumonia, 29 at the right side, 24 at the left, 17 in both lungs.

Lobes.—Inter-vesicular Inflammation in the 13 cases without suppuration, 10 in the inferior lobe, 1 in the superior, 2 in both lobes; in the 5 cases with effusion of pus, 4 in the inferior lobe, 1 in the two lobes.

Vesicular Inflammation in the right lung, 15 times the inferior lobe, 10 the superior, 4 the two lobes.

In the left lung; 5 the inferior lobe, 17 superior, 4 two lobes. As to the surface of the lung attacked, we find by an examination of facts, that the *inter-vesicular* engorgement constantly occupied the posterior surface; while the *vesicular* form was found 12 times near the anterior surface, and 25 the posterior. These numbers demonstrate in an evident manner the influence of the reclining posture in producing pneumonia, at least in old persons.

State of the Bronchi.—In all cases the bronchial tubes are red; the coloration often extends so far as the epiglottis, and follows the finest ramifications of those canals; however, we should remark, that no matter what may be the cause of death, the bronchial membrane in old females almost always presents more or less general redness.

The Trachea and Larynx are also filled with a great quantity of mucus; this secretion is generally viscid, thick, of a dull white or yellow colour; and in some cases the

large bronchial tubes are filled with a mixture of blood and mucosity. The air-tubes also frequently contain a quantity of irregular coagula formed by a white concrete matter, but their history belongs more properly to that of catarrh.

State of the Pleura.—In the 70 cases of vesicular pneumonia: the state of the pleura was not noticed in 10; 22, no pleurisy; 38, pleurisy evinced by the presence of recent false membrane. In these 38 cases, the pleurisy occupying in 18 cases the left side, coincided sixteen times with pneumonia of the same side; 2 with double pneumonia; 14 times at the right side; coinciding 13 times with right pneumonia, &c., with inflammation of the left lung.

An Authentic Report of an Introductory Lecture on the Important Subject of Medical Reform; delivered in the School of Anatomy, Peter-street, Dublin. By ANDREW ELLIS, Esq., Professor of the Principles and Practice of Surgery, &c. Fannin and Co., Dublin, 1836.

Two very opposite opinions are entertained of the proper contents of Lectures of this description. One of these opinions would exclude from such discourses all controversial matter, and confine them to the exclusive consideration of scientific subjects. The other admits the propriety of discussing the general economy of the medical profession, as well as its immediate objects. The objection to medico-polemical addresses is founded, we believe, upon the presumed impropriety of discussing exciting topics in assemblies of young persons, whose only business, it is said, should be the attainment of scientific knowledge. We have certainly read and heard some very beautiful "lay sermons," in which this argument was developed with as much ability as philanthropy. It is also a favourite theme, with, and eloquently treated by, the "optimists" of the medico-conservative press, who can discover nothing so bad in the condition of the profession as the innovating spirit of some members who would "disturb the present harmony of its parts, and set its junior followers in opposition to their superiors." It may, however, be fairly suspected that there are other and still more powerful reasons for thus opposing the practice of examining the laws and constitution of the profession in the presence of medical students. The "Quietists" do not give us the benefit of a full and unreserved view of their thoughts upon this delicate question. They pour out before us with the utmost freedom the clear and specious portion of the argument; but they take care to lower their hand when they approach the feculence that rests at the bottom. It is more than probable, indeed, that this desire to preserve the constitution of the profession from the gaze of youthful novices may be intimately connected with a conviction that its character is not of that pure and spotless kind which would bear inspection and inquiry. Were there no defects in the system, the advocates of concealment would not be likely to fall into the inconsistency of proposing to prevent a scrutiny, which, if there were nothing wrong to be detected, could only demonstrate the errors and misrepresentations of those who hold the opposite opinion.

Mr. Ellis is certainly not one of those who thinks that any injurious consequences can flow from a free investigation into the state of the medical profession. It is difficult, indeed, to understand how a knowledge of the absurdities of the medical code, or of the abuses of the medical corporations, can be considered as incompatible with the attentive dissection of a muscle, the performing of a chemical experiment, the diagnosing of a disease, or any other of the various duties with which students are occupied. Why should it make a pupil more negligent in his studies to be told that at one of those exhibitions called an examination for "letters testimonial" in the Dublin College of Surgeons for instance, the son of one of the Professors of that institution on being asked from what part of the plant opium was derived, replied that it was extracted from the root! Neither can we see in what manner it can impede his progress to be informed that this intelligent youth, after giving this answer, was forthwith honoured with a diploma by some dozen of examiners, sworn to discharge their duties efficiently and impartially. Nor are we able to discover any serious injury which can accrue to the learner by hearing that the young "Crichton" who could thus obtain opium from the root of the poppy, was, in a very short time after he made this notable discovery in medical botany, appointed to one of the first situations in the medical profession in Ireland, the surgeoncy to *Stevens' Hospital*, in Dublin.

Upon this hypothesis, at least, of the innoxiousness, if not of the utility of disclosing without reserve the "secrets of the prison-house" to students, a great portion of Mr. Ellis's Lecture is founded. His conclusions, indeed, in favour of a reform of the medical code, are based upon the most comprehensive and fearless examination of its enactments. Universities, Colleges, Halls, Hospitals, and the various forms of medical institutions in these countries, are each in its turn put into the witness-box, and made to give the most abundant and satisfactory evidence of their abuses and erroneous constitution. Their charters, by-laws, usages, and regulations, are severally examined and compared in conjunction, so that "he that runs may read" in the narrative such a mass of absurdity, and so appalling a picture of human abuse, as was never, we believe, before presented of any social establishments, whether civil or scientific. We shall select a few of the striking features which make up the ludicrous, but, at the same time, humiliating portrait of the medical corporations, as presented to us by the pencil of Mr. Ellis. Speaking of the Universities he says:—

"By a perusal of the rules and regulations which have been issued by the Dublin and Edinburgh Universities for many years, it may be seen that they required, till very lately, but a 'six months' certificate for hospital attendance, from candidates for the degree of M.D., whilst those of Glasgow and St. Andrew's demanded twelve months; and nothing less than three years would satisfy the fastidious law-makers of Aberdeen! Does not this circumstance in itself amount to a satisfactory proof, that some of these institutions must be wrong in their mode of acting in this respect; and that there is an obvious necessity for the interposition of legislative interference, with a view to strike a proper average, which should be common to them all?"

This average should amount to *not a fraction of a day*.

It is pleasant to hear what strangers say of our own virtues or follies. In his observations on the different usages of the Universities with respect to the language in which examinations are conducted, Mr. Ellis remarks upon the practice of that illustrious body in Pall-Mall-East, of which Sir Henry Halford happens to be the head, thus:—

"What can be more ludicrous to human conception, than to imagine two persons discussing in a language with which, in all probability, they are but imperfectly acquainted, some abstruse point of science, of which they perhaps know less? Yet we find that very classical and aristocratic body, the Royal College of Physicians of London, still adhering, with a superstitious fidelity, to the old but now nearly departed path of folly, which has been too long trodden by medical corporations, under the mistaken notion that it imparts learning and dignity to its blind followers!"

As an exemplification of the evils of different systems of education being in use in the different Universities, and of the practice prevailing in those establishments of receiving their own certificates alone from each other, Mr. Ellis asks,

"Is it not a great hardship that my apprentice, who has an opportunity of learning anatomy and physiology in this school, free of expense, must, because he is desirous of taking out a medical as well as a surgical degree, pay four guineas to Dr. M'Cartney, merely for the privilege of being incarcerated for six months in the anatomical lecture-room of Trinity College?" "Well," continues Mr. Ellis, "this is not all, for university professors are not only the teachers of medical students, but likewise the examiners, and the persons who grant degrees to their own pupils! What can be said in favour of a system, of which this very objectionable procedure constitutes a prominent part? Do you think it accords with our idea of the kindly feeling of human nature, to suppose that teachers could bring themselves, in the capacity of examiners, to act impartially by voting against their own pupils, and in this way injure their future prospects in life? The thing is next to impossible; the more especially, as the examiners themselves must know very well that *their* pupils had not a fair opportunity of learning their profession."

The measure which Mr. Ellis deals out to other institutions, he impartially applies to his own. He thus speaks of his own "Alma Mater;" and we beg the reader to remark, that the speaker is not a person unacquainted with, or ill-disposed to, that establishment, but a gentleman who has himself performed the duties of examiner there. Of some points in the economy of the Dublin College, he thus speaks:—

"But, nevertheless, I regret exceedingly that I cannot claim for the Irish College, of which I am an humble member, that degree of perfection and of public approbation which I fondly wish it was entitled to enjoy. We have fallen into the deplorable error of the universities, in having a school attached to it, and to this unfortunate connection may

be fairly attributed whatever dissentions may have arisen amongst us. It would ill become me to speak disparagingly of either that school or its conductors; but at the same time, I feel it would be a culpable affectation of delicacy on my part, if I did not openly express here, as I have done elsewhere, my objection to the principle of discord and monopoly involved in the fact of a school constituting a part, either of a medical or surgical corporation. It might be urged as an argument in favour of the continuance of this school, that the professors must be efficient teachers, inasmuch as they are appointed by members of the body who are, it is to be presumed, competent to form a correct judgment of the qualifications of the candidates. Here, I feel, that in order to render this question intelligible, it is necessary to apprize you, that the members of the Dublin College of Surgeons are, in common with those of most corporations, divided into two parties, which earnestly support their respective sides on all important questions of college politics. Such being the case, the annual election of officers is determined by the strength of the predominant party. Accordingly the Court of Censors consisting of six members and the president, with whom the election rests, must be looked upon in the light of a fortunate or favoured few, who owe their official rank to the support of certain individuals. This is a kindness not easily forgotten by the Censors, who are in gratitude bound to return the compliment, as far as the extensibility of the consciences of corporate officers will permit, by appointing to the professorial chair, the candidate who may have the good fortune to be a favourite with a certain party. Furthermore I beg to add that four of the Censors, together with the president or vice-president, constitute a court, and the majority of such a court have the power of nominating a professor."

So much for the mode of election in the "first surgical College in the world," by which it appears that a majority in a division of the magnificent number of seven electors are able to return Arthur Jacob, or a chimney-sweeper, as the representative of surgical science in Ireland! Let us see how these self-elected Censors, Professors, and Examiners, for these names seem to be almost synonymous, act towards each other and the public.

"Such professors of the College School, as are members of the corporation, are eligible to be elected on the Court of Censors; and, consequently, they may have not only a voice in the election of each other, but likewise the power of examining and voting for or against candidates for 'letters testimonial.' The professors and their friends have studiously availed themselves of their privileges in this respect; accordingly I believe there has not been a single year, for the last twenty or upwards, in which one, two, or more, of the professors were not members of the Court of Examiners! This, I think a very unfortunate circumstance as regards the reputation of the College. Only think of men electing themselves and their colleagues; and examining and granting diplomas to their own pupils! A court of censors should be in my opinion above suspicion; and they never can, so long as they are permitted to examine either their own pupils or those of rival teachers."

Passing over much pertinent matter, we come to Mr. Ellis's remarks on the appointments to hospitals in Ireland, one of which remarks we cannot resist quoting, as conveying the substance of his experience and opinion upon this subject.

"With the electoral bodies (of hospitals) I have just mentioned, it is a well-known fact that their decisions are more frequently determined by money, and feelings of a private or personal nature, than by the attainments or the moral worth of the candidates. By way of illustrating this, permit me to apprize you of a few questions which were put to me, by the chairman of the Parliamentary Medical Committee. One of these was to this effect:—'Is it not notorious that the elections for masters which have taken place from time to time in the Lying-in Hospital in Britain-street, have been corrupt?' *Answer:*—'Judging from what I have heard since I came into this room, they must have been so!' I was subsequently asked if Mr. Crampton and four of his apprentices did not constitute five out of six of the surgeons to the *Meath Hospital?* Here I set the honourable gentlemen right by stating that, 'As yet, *only three* of Mr. Crampton's apprentices had been appointed surgeons to the hospital!' Mr. Crampton's prowess as a surgical sire, from this, would appear to be undoubted; and that his paternal care in providing for his scientific offspring, is fully equal to his powers of procreation, is quite manifest from this little family anecdote of himself and his apprentices!"

We have thus, we presume, quoted sufficiently from this discourse, to give the reader an idea of its contents, which include almost every subject connected with the present degraded state of the medical profession and the vicious condition of our medical code. As a brief but faithful view of subjects which are at present occupying the attention of the legislature and the profession, we can earnestly recommend it to the attention of the public. In con-

clusion we have merely to observe that it contains Mr. Ellis's opinions of the introduction of the "Concours" into this country; and a correct outline of the manner in which that valuable institution is worked among our continental neighbours, the French.

THE LANCET.

London, Saturday, April 30, 1836.

In the new arrangements of the College of Physicians, a more striking proof could not be furnished of the manner in which the interests of the profession were neglected by that body before the Apothecaries' Bill of 1815 was enacted into a law. The charter of this College conferred upon its managers the power of exercising the most important privileges of a medical police; but in their love of a restrictive aristocratic monopoly of *title* and fulsome *pomp*, they were unmindful of the powers of the law, and of the privileges and interests of the great body of the profession. The acknowledgment which is made in the new regulations, of the extent of their delinquencies, will obtain for them neither the pity nor the respect of their professional brethren. The College of Physicians has long been virtually defunct.

At no distant day an efficient Bill for regulating the office of Coroner must be introduced into the House of Commons. All the measures which have already been brought forward with that object, have been shapeless masses of incongruity. Instead of aiming at the great principles which should regulate the Court of the Coroner, they have all sought to accomplish some minor and subordinate objects, which have operated rather as sources of inconvenience to the persons acting in the Court, than prejudicially to the interests of public justice. The Legislature of England is deemed, we believe, to be one of the most enlightened in the world. Yet, in fixing the qualifications of individuals for important public offices, a rule more obnoxious to every principle of reason and justice, cannot be observed than the one which is followed by our Houses of Parliament. In order to render a man capable of discharging the duties of a magistrate, he is required to have a certain amount of money in his pocket. If there be not a proportion of wealth, the law, both in spirit and letter, implies that the individual has not the power of deciding justly and wisely. Money is received as the best evidence of intellectual capacity and moral worth, and the rich man is placed on the seat of judgment, *because* he possesses wealth, making the *mode of attaining it* a subject of no consideration. In our Select Vestry Acts, the qualifications for voting are made to depend on the amount of assessments, and when a man is rated at 150*l.* a-year, such is the effect of this fiscal regulation, that the law presumes that his mind is endowed with the highest degree of sagacity, and possesses the most powerful inducements to pursue an incorruptible course of conduct. He may have acquired his rateable property by extortion, usury, or swindling. Still, this is of no consequence. He is enabled to neutralize the votes of six honest men, whose property, taken in the *aggregate*, doubles his own, and who happen, *individually*, to be poorer than himself, for no other reason than because they are more honest.

Again, in the law relative to the Parliamentary elections, we find that the qualification of the voter is merely an affair of pounds, shillings, and pence. The ignorance of the elector is no bar to the exercise of one of the most important privileges in a free nation. If a party be capable of paying ten pounds a-year rent, he is acknowledged at once to be a marvellously clever fellow, and considered to be singularly well qualified for testing the capacities of candidates for seats in the Legislature. If his

rent amount to only nine pounds, nineteen shillings, and elevenpence three farthings, the law treats him at once as a slave and a fool, and ejects him beyond the pale of the constitution.

Such are the wise provisions of the law concerning what are regarded by the Legislature to be the qualifications necessary for executing the duties of important trusts. As we advance on the road of improvement, new and more rational views will open for our guidance. If the choice of electors is to be restricted by certain specified qualifications, if the number of the electors themselves is to be limited by any arbitrary provisions of the law, assuredly common sense and prudence would point to the propriety of requiring those qualifications which would best enable possessors to discharge the peculiar duties of their respective offices. If, for example, a Bill for regulating the office were again brought forward in Parliament, would it not be infinitely more prudent to substitute for the present paltry and ridiculous *property qualification* in the Coroner, one founded strictly on an acquaintance with *the principles and practice of medicine?* This would be an *intellectual* qualification, and would furnish the best *prima-facie* evidence of the coroner's capabilities for fulfilling the important obligations of his office.

Unwilling, as we are, to encourage a meddling system of legislation,—disinclined as we are to urge interferences of the Legislature in any case where the people are capable of exercising important privileges which have been handed down to them by their forefathers, by prescriptive sanction and authority, yet we are inclined to apprehend that the appointment of non-medical persons to the office of coroner will long be continued, to the great detriment of public justice, unless Parliament shall ordain, by a statutory enactment, that a diploma, or license, obtained from a medical corporation, shall be deemed an indispensable qualification for every coroner. Often have we stated, that it would be better to abolish the office of coroner altogether, than to allow non-medical persons to exercise its functions.

We observe, with regret, by an article in the *Liverpool Journal* of April 23, that the example and influence of such a physician as Dr. Carson, have failed in the town-council of Liverpool, to obtain from that body a sound decision in the appointment of a coroner. We should have supposed that the members composing the town council of such a place as Liverpool, would have been governed in their selection of an important public officer, by the ordinary principles of reason and justice. The following paragraph taken from the journal in question, announces the adoption of a course as contrary to the dictates of sound sense and propriety, as it is obnoxious to the welfare, and opposed to the good government, of that portion of the community which has fallen under the municipal care of the Council:—

"The appointment of coroner took place on Wednesday, in favour of Mr. P. F. Curry, 'one of the solicitors to the Liverpool Reform Association,' as the town-clerk announced him to be when his letter of application was read. The Council negatived a resolution (by Dr. Carson), that a medical man was the best suited to fill this situation, and yet recognised the principle, by the appointment of a gentleman, whose *second* claim was that he, as an amateur, had a smattering of medical knowledge. Thus, they reversed the doctrine of Pope, that 'A *little* learning is a dangerous thing.' For the Council practically declare, that although to be a medical man, out-and-out, is no qualification for the coronership, yet it is all the better that there should be a *little* medical knowledge."

M. Broussais lately commenced a series of lectures on *Phrenology*, in the large amphitheatre of the school of the Faculty of Paris. The confluence of auditors from all quarters of Paris was immense. On the 15th inst. the concourse was so great that M. Moreau could not terminate his lecture, the pupils having forced the doors of the lecture-room half an hour before the expected arrival of M. Broussais. We had reason to fear greatly that this conduct

would only serve to interrupt the lectures, which we had taken measures to publish in full.

Since the above paragraph was in type, we regret to say that M. Broussais, as we expected, has been compelled to suspend his course of lectures until a more capacious amphitheatre than that of the School of Medicine can be found for the delivery of his discourses. In case he resumes them, we shall not fail to publish an account of the great physiologist's lectures, which, contrary to what happens in so many cases, he was unable to continue from the superabundance of auditors.

We forgot to announce last week a discovery which has made a great deal of noise in the French capital. The natural cow-pock, so long sought after in vain, has at length been discovered at Passy, a small village, about a quarter of a mile from Paris. Unfortunately, the existence of the eruption in the human subjects was not made until the pustules were quite dried in the mamma of the cow which infected them; but concomitant circumstances leave little doubt of the reality of an eruption of cow-pock having been transmitted from a brute animal to the human subject.

The concours for the chair of anatomy of the Faculty of Medicine commenced on the 14th inst. The candidates were, M. Breschet, M. Blandin, M. Broc, M. Laurent, M. Jobert, M. Lebaudy, M. Chassaignac, and M. Michon.

MIDDLESEX HOSPITAL.

AMPUTATION AT THE HIP-JOINT FOR NEURALGIA!

To the Editor of The Lancet.

Sir,—As the interests of humanity have ever found a warm and powerful advocate in your pen, I trust that the above operation, performed under circumstances almost without a precedent, even in the annals of *hospital surgery*, will not be suffered to pass away in silence. It is owing to you, Sir, and to your exertions, that hospitals have been at all restored to the original intentions of their benevolent founders, that the poor can venture to look up to them in any way as their asylum when wounded or diseased, instead of being converted into an arena for the exhibition of heartless butchery, in which men "cut their way" to the temple of Fame; but I fear that you have "scotch'd the snake, not killed it."

The unfortunate patient to whose case I implore your attention, had previously undergone *three operations* without having experienced the slightest relief. I will not dwell on the judgment displayed by Mr. Mayo, in amputating the limb in the first instance, for a neuralgic affection. *That* may be a subject for further consideration. All that I desire, at present, is to call your attention, and that of the governors of the institution, to the performance of so tremendous an operation, upon so hypothetical a chance of relief, and after the senior surgeon, Sir Charles Bell, *had protested against it*, and had declared in a letter to Mr. Arnott, *that if the operation were performed it would be a disgrace to the institution*, and after the other surgeons, Mr. Arnott and Mr. Tuson, had declared that they would not, under the circumstances of the case, have performed it. But Mr. Mayo said, "I will." I know that the defence attempted to be set up is, that "the poor patient desired it;" but this shadow of a shade of an excuse will not hold good for an instant, when I declare to you that for months and months the most specious arguments and solicitations have been employed to obtain the poor girl's consent, and I do not think that any one will venture to assert that the dangers of the operation, and the months of acute suffering which will inevitably follow it, even under the most favourable termination, were ever pointed out to her.

Trusting that you will make full inquiries into the case, the full particulars of which, including its previous history, I will forward to you if you do *not* decline them in reply to this offer,* I remain, Sir,

Your obedient servant,

Humanitas.

April 23, 1836.

REMUNERATION OF MEDICAL WITNESSES AT INQUESTS AND TRIALS IN SCOTLAND.

To the Editor of The Lancet.

Sir: During the whole of your very meritorious exertions for obtaining remuneration for medical practitioners when called to give evidence before the coroner, it has appeared to me very extraordinary

* We do not.—Ed. L.

that no allusion has been made, either by yourself or any other person, to the manner in which medical men are dealt with under similar circumstances in Scotland.

We have no coroners, the Procurator Fiscal of each county performing the same sort of duty as the Coroner in England, but without a jury; and on every occasion when medical evidence is required, at least two medical practitioners are called before him, and each receives one guinea for his trouble, no matter how short a time he may have been detained. If a post-mortem examination be required, then each receives an additional guinea for his services. They are also allowed travelling expenses when any have been incurred. When the medical witness has afterwards to appear at the trial of the accused, he again receives a guinea: and whenever he is called beyond the limits of the county wherein he resides, he is paid two guineas for each day he may be from home, in addition to his travelling expenses. For instance, when a prisoner, as is sometimes the case, instead of being tried at the assize of this county, is tried before the High Court of Justiciary in Edinburgh, the medical witnesses are each allowed *eight guineas* besides their travelling expenses, although they are only absent from home *three days*, the distance being seventy-two miles; the fourth day is paid for, because it is supposed, and justly, that the day immediately preceding the one on which the witness leaves home, is in some measure lost to him in so far as regards his profession, as it would not be safe or prudent for him to go far from home on that day, or to engage with any case that might detain him beyond the proper time for setting out upon his journey.

I am, Sir, yours very respectfully,

ARCHD. BLACKLOCK, Surg.

Dumfries, April 12, 1836.

ANOTHER DEATH FROM THE ADMINISTRATION OF MORISON'S PILLS.

To the Editor of THE LANCET.

SIR: I was desired by the friends to see a female infant, aged 17 months, on Wednesday last, under the following circumstances:—On inquiry I found that up to the period of weaning (thirteen months) she had enjoyed good health, and was a fine ruddy-looking child, the mother having occasionally administered a little castor-oil. About that time the father, from some benefit which he states he had himself derived from taking *Morison's Pills*, and from having also administered them to his elder daughter from the time of her weaning, and from having seen numerous reported cases in a publication called the *Medical Dissenter*, in which it was stated that the same medicine had been given to infants with signal benefit, and a belief that they would preserve health, as is impudently asserted by the quack, began to administer them to the child in quantities at first varying from two to four in the twenty-four hours; but afterwards, by the child being ill, the dose was increased by the advice of the notorious Mr. Salmon, to twenty-two in the course of the twenty-four hours. The child had been so much reduced that she appeared, when lying in bed, to be of the age of six or seven months. The usual effects of the pills have been to produce vomiting soon after being taken, and, afterwards, copious, yellow, mucous, muco-purulent, and bloody stools. The "*agent*" in this neighbourhood, *an old Scotch midwife*, recommended the parents to give the child another dose on the night before the child died, and though I prognosticated the case to be almost hopeless, the "agent" again urged them to be taken a few hours before death, declaring that the child was doing well.

On the Friday following the child died, after being convulsed for four hours.

With the permission of the father, and in order to convince him of the danger of using indiscriminately such a powerful medicine, I made a post-mortem examination. The body was much emaciated, and the abdomen tympanitic. On opening the chest, an adhesion was found of the superior lobe of the right lung to the sternum and ribs, and in the upper part of the lungs were some softened tubercles, and throughout both lungs there were interspersed some small miliary tubercles. There were about two ounces of serum in the pericardium; the heart was sound; there was a slight adhesion of the liver to the abdominal muscles and the diaphragm; that organ was healthy; the gall-bladder full of bile; the spleen and pancreas were sound; the bowels presented the appearance of being coloured by dark substances within, and the large intestines were stained quite yellow; there was no adhesion of the peritoneum; on lifting up the stomach gently, to tie up the œsophagus, my finger passed readily through, and the contents escaped, and on slitting it open I found three large ulcers, which had destroyed the mucous and muscular coats. In the course of the intestines, there were twenty-seven more ulcers, varying in size from that of a silver penny piece to a shilling; the mucous membrane was so softened throughout, that it could be scraped off like paste, with the back of a knife or with the nail; the kidneys were healthy; the bladder was much contracted.

This is I think, without doubt, another instance of the destruction of a victim to the insatiable avarice of the health-destroying quack Morison, and, although from the appearances in the lungs, it was not pro-

bable that the child could have lived to maturity, yet on that very account, such highly drastic purgative medicines must have exerted a more speedy and prejudicial influence on life. I have thought it my duty to communicate this case to you, and remain, Sir, your very obedient servant,

GEORGE TUCKER.

2, Cardigan-place, Kennington-cross,
April 27th, 1836.

*** A coroner's inquest ought to be held on the body of this child.—ED. L.

MORISON'S PILLS.

To the Editor of THE LANCET.

SIR: As I think that every proof of the fatal effects of *Morison's Pills* ought to be laid before the public, when any clue to them can be obtained, I send you a few lines to say that, on Sunday last, a gentleman was buried from No. —, Westmoreland Place, City Road, from the respectability of whose funeral I was led to inquire the particulars of his disease. The name I cannot learn, but I was told that he came to town some few weeks before his death, that he had been taking Morison's pills, and that both his doctor and his widow assert that the use of those pills has occasioned his death.

I consider this case to be worthy of publicity, and should the necessary inquiry be made, I doubt not that such facts will be elicited as will further warn the public against the poison sold under the name of *Morison's pills*. I remain, Sir, yours respectfully,

A. F. M.

King's Square, April 14, 1836.

EPPING UNION.

LETTER OF EXPLANATION.

To the Editor of THE LANCET.

SIR: When we observed the notice you had taken of the meeting of medical men in the Epping Union, we resolved to leave our conduct (so much misrepresented) to the conscientious judgment of our medical neighbours, some of whom composed that meeting. But as we have found that your useful journal is as much read by non-professional persons as by those whose profession it benefits, we consider it due to our character to explain away the misrepresentations which you have recorded against us.

A meeting was held at Epping some two months since, at which it was agreed (according to our *recollection*, as no minute was taken of the proceedings) that no medical man present would act contrary to the interests of his professional neighbours, in tendering for attendance on the poor, or attending any meeting of the Guardians that might take place. Now the formal resolution said to have been passed, must have been composed after that meeting, since nothing was written during our conference, nor during a champagne dinner which took place afterwards. Some time after this the offer of the Guardians for medical attendance on the poor was made, and we (considering it would be equally acceptable to others as to ourselves, in lieu of the small sum we had hitherto received for parochial attendance) at once accepted the offer for those parishes which we had attended between fifty and sixty years, and we thought that by so promptly accepting their offer, we had a chance of baffling an opponent, who a short time since obtained a parish at six miles distance, in a neighbouring union, by tendering for it at five per cent lower than the medical man living in that parish. And now, Sir, we will answer the falsest part of your correspondent's insertion, respecting "*our friend* Mr. Rix."

In our letter accepting the Guardian's terms, we stated "that should no more eligible offer be made, we should be willing to take the second district," comprising three parishes, one of which we held, and that one larger than the other two together. The same day that we communicated with the Guardians we wrote to Mr. Rix, stating that we had accepted the tender of the Guardians for our district, and mentioning our reference to the second; we observed at the same time that we considered him more eligible, and of course that the Guardians would think the same; also that we should be happy if he would take it. *He consequently wrote to the Guardians, and accepted their tender*; he was appointed surgeon to that district, and afterwards declined the office. Upon his refusal we were appointed. Mr. Rix has the power of contradicting this assertion, or of publishing our letter.

We beg, to apologize for thus troubling you, and to state that we feel assured that that justice which you have ever shown in exposing professional malpractice, will induce you to publish *our explanation* of the proceedings of the medical men in the Epping Union. We are, Sir, yours obediently,

DOBSON and GODDARD.

Harlow, Essex, April, 21, 1836.

To the Editor of THE LANCET.—SIR: Much having been said in different publications, on the properties of Mr. Franks's specific for gonorrhœa, I beg, for the information of yourself and your readers, to refer you to Sir A. Cooper's lectures, delivered at the Borough School, twenty-five

or thirty years ago, where he recommends for the cure of gleet &c., balsam of copaiba and nitric ether, which, in my belief, is the combination that Mr. Franks has furnished to the public for the cure of this disease.
Your obedient servant,
M. Rowe, M.R.C.S.
24, Woburn Place, April 21, 1836.

ILEO-VAGINAL AND RECTO-VAGINAL FISTULA.

At the last meeting of the *Westminster Medical Society*, Mr. Thurnam of the *Westminster Hospital* related the following case. A woman, aged 37, was admitted into the hospital about a month since; suffering from a constant watery diarrhœa and extreme emaciation. It was stated, that about four months before she had been delivered by instruments of a fœtus, which, it was said, had been dead for some days. Since that period she had suffered from great pain in the hypogastric region, and profuse diarrhœa, which had reduced her to a state of extreme emaciation; nothing had been done, except the application of a dozen leeches: in this state she was admitted into the hospital. She did not complain of any local pain. As the nurse reported the feces came from the vagina, Mr. Thurnam carefully examined that canal, expecting to find recto-vaginal fistula; this he did not do; he was fearful of using the speculum, from the great degree of exhaustion the patient laboured under. The diarrhœa continued in spite of all remedies, and she gradually sank, and a few days since died. On examination of the body, the cavity of the peritoneum was found free from fluid. The fundus uteri was adherent to the sacrum; the adhesion involved the left ovary, fallopian tube, rectum, and several convolutions of the ileum. The ileum and rectum were pressed against the sacrum by the fundus uteri. The feces had evidently not passed for a long period; there was a collection of them in the rectum, the whole of the large intestine, and the termination of the ileum, were remarkably contracted, and contained a thick, glutinous, and grayish mucus. A number of fistulous openings were detected at the upper and right side of the vagina, communicating with the inferior part of the ileum. There was also a small fistulous opening into the rectum. The mucous membrane of the vagina was in a state of sloughy ulceration. What would be the consequence of mistaking such a case for one of common recto-vaginal fistula, and its treatment by actual cautery, as recommended by Dupuytren? Could the ileum, contracted as it was, ever regain its function of transmitting the feces?

Dr. Johnson thought the only likely means of relieving such a case was by removing the feces by means of injections &c., and thus give the fistula a chance of healing.

A long speech was afterwards made by Dr. Granville, on the impurities of the Thames water, in which he descanted with considerable eloquence on the horror of our drinking so much filth as was contained in it; for neither filtering or subsidence would have the effect of removing the essential salts which formed the worst parts of such filth.

He was followed by our friend, Dr. Johnson, who with many sighs and looks indicative of great emotion spoke of the sufferings he had experienced, when he resided in Spring-gardens, from his servants being too lazy sometimes to fetch other water than that which was supplied to the house, which was from the Thames. He had, in consequence of this, removed to Suffolk-place, his present house being supplied from the New River.

ACETATE AND CARBONATE OF LEAD.

At a late meeting of the *London Medical Society*, the discussion turned on the therapeutic agency of the preparations of lead, and the manner in which it acts upon the system, or on particular portions of the body, in producing paralysis. Mr. Dendy said he had prescribed the acetate of lead in hæmoptysis, in the case of a delicate young lady, from whom large quantities of blood had been abstracted, in doses of ten grains, three times daily for several days, without any perceptible effect resulting from the dose. Mr. Howell observed, that he remembered a case in which a man who wished to commit suicide, had swallowed two drachms of the acetate of lead in half a pint of ale, without any bad result following. — Mr. Bryant said he had been informed of a case in which the patient had swallowed twelve drachms of the same medicine without injury. A long conversation then took place on the cause of "dropping of the hand" in painters, and on the influence which lead has in its production. A variety of opinions were expressed on the subject. Mr. Bryant thought, that an opinion given by Dr. Burne, which attributed the effect to the muscles being kept constantly on the stretch by the use of the hand in painting, was correct, the muscles not allowing the hand to return to its natural position. Mr. Dendy was also inclined to hold this opinion. He had seen the "dropping of the hand" in water-colour painters, who had used no lead. Mr. Pilcher said he had examined a patient some years since in *St. Thomas's Hospital*, who died from colic, in whom carbonate of lead was distinctly found in the extensor muscles.

of the arm. The supposed lead, however, had not been analyzed; but Dr. ELLIOTSON, whose patient it was, concluded that the substance found was carbonate of lead. With regard to the treatment of the affection, Mr. Pilcher said he had found constant blistering of very great service. Dr. L. STEWART had seen many parts paralyzed from the local application of lead, particularly from the use of Goulard's extract. He had seen the penis paralyzed from using that preparation. Dr. JOHNSON observed, that he had used the acetate of lead for thirty years in pretty large doses, and had never seen it do harm. Obstinate constipation had in many cases come on, but this might, perhaps, be attributed to the opium conjoined with it. A gentleman present mentioned that he had seen the acetate of lead in two cases produce ptyalism, the breath having a peculiar "sour-sweet odour." It was mentioned by one or two gentlemen in the course of the evening, that old painters, who work with their arms covered up, and who take great care of themselves, by keeping the flesh clean, &c., are not subject to paralysis, whilst the younger ones are very liable to the affection.

TESTS FOR THE SALTS OF IODINE.

Dr. A. T. THOMSON at a late meeting of the *Westminster Medical Society* introduced some new tests to detect the presence of iodine in any fluid or secretion in which a decomposition had not taken place, or in which hydriodate of potash was formed, into which salts he considered that all the preparations of iodine were converted, when taken into the circulation, by some process which he could not pretend to explain. The nitro muriatic acid and the nitrous acid were the substances which would detect iodine in the smallest quantities. Into the fluid to be tested a small quantity of solution of starch was poured, and a few drops of either of the acids above-named, if added, would immediately precipitate the iodide of amylum, characterized by its beautiful purple colour. Dr. T. experimented on several liquids containing the hydriodate of potash in exceedingly minute quantities. Water, milk, infusion of tea, all exhibited the test in a very beautiful manner. Chlorine and the nitrous acid gas would, he said, produce the same results; but these, from their difficulty of attainment, would not be so generally employed; neither were they so easy of application. Chlorine gas possessed one remarkable advantage over the liquid chlorine, for if the last was added even to the extent of a drop beyond that which was necessary, the precipitate would be dissolved and the test would be fallacious; while if used in the gaseous state, as it only came into contact with the surface of the liquid, there could be no fallacy, as the effect was immediate. These tests, he added, possessed advantages over all others, both from their simplicity and delicacy, and he doubted not that they would be found invariably infallible. The tests exhibited and detailed were very delicate and beautiful. Dr. Thomson mentioned that he had detected hydriodate of potash in the urine, the saliva, and the perspiration. Mr. Bird said that he had also detected iodine in the blood.

NORTH-LONDON HOSPITAL.

RUPTURE OF THE SMALL INTESTINE FROM THE KICK OF A HORSE.—DEATH ON THE 7TH DAY.

RICHARD ALCOCK, ætat. 34, a fine healthy man, admitted under the care of Mr. COOPER, at about five o'clock in the afternoon of 13th of April. It was stated that a short time previous to his admission he had received a severe kick from both feet of a horse which he was in the act of dressing, upon the hypogastric region. That he fell from the force of the blow, and became immediately sick, rejecting the contents of his stomach. He also passed a copious evacuation from the bowels immediately after the receipt of the injury. No blood was noticed in the stool. The man had made water immediately before the occurrence of the accident. To use his own expression, "the blow quite doubled him up." At his admission he was in a state of complete collapse. Pulse almost imperceptible; countenance sunk, skin bathed in a clammy perspiration. The vomiting still continued and was incessant. There was no ecchymosis or other external evidence of the injury which had been received; but the belly was swollen, tense, and very tender when pressed upon, especially towards the hypogastric region. He had not been long in the hospital when he passed water freely, no blood in it. Warm fomentations to the abdomen, and an *effervescing* draught, with some tincture of opium to allay the sickness.

April 14. Has passed a very restless night; harassed by constant vomiting of a greenish-coloured fluid; pain, tension, and tenderness of the abdomen, are increased. Skin and conjunctiva have assumed a yellowish tint.

Mr. COOPER saw him, and ordered him to be bled to twelve ounces; twelve leeches to be applied to the abdomen; followed by fomentations.

The pulse, which before the bleeding was 120 and distinct, became very feeble, and increased in its number to 130. He felt very faint. Passes water freely, and which is high coloured.

Vesperé. Blood buffed and cupped; feels somewhat easier; but his sickness remains as urgent as before. Ordered one grain of opium every hour.

April 15. Appears much better; has slept for several hours; anxiety of countenance is very much less; tongue moist, but covered with a yellowish fur; thirst; pulse 120, and of moderate strength. At two in the afternoon he became very much worse. Tension and tenderness of belly increasing; pulse is small and indistinct. Vomiting, which had subsided somewhat, has become as urgent as ever. To have two grains of calomel and half a grain of opium every half hour.

16. Next day he was much the same. Constant vomiting of the same greenish-coloured fluid, with frequent hiccup. Pulse 120. The leeches to be repeated, and a large blister to be applied to the abdomen.

April 17. Appears to be more easy to-day, though he has passed a restless night from the vomiting. Pulse 118; countenance varies; at times more anxious than at others, and is then covered with a clammy sweat.

Vespere. Is sound asleep. During the course of the afternoon he took several doses of creosote, ℥|x in each, which had a very decided influence in relieving the vomiting; before its exhibition the vomiting was incessant; he is now sick about once in two hours.

18. Appears better; pulse 90; tongue clean; tenderness of abdomen is less, but the distention remains as before, and there is an obscure sense of fluctuation; mouth is becoming affected by the calomel.

Six p.m. Has changed suddenly for the worse. Appears to be dying; extremities and nose quite cold; countenance shrunk. A glass of wine frequently. Baths of hot water to the extremities.

19. Vomiting has returned as severe as at first; the colour is gradually assuming a darker tinge; pulse almost imperceptible at the wrist. The appearance of the patient is exceedingly similar to one suffering under an attack of Indian cholera, with the exception of the blue tint characteristic of that disease. As no stool had been passed since the occurrence of the accident, an injection of a pint of warm water and a little castor-oil was slowly thrown up the rectum. In the course of the day it brought away several copious feculent evacuations. To have two minims of creosote every hour. To have brandy frequently; beef-tea ad libitum.

20. He appeared to rally a little at times during the latter part of yesterday, not being so urgently sick; but to-day he is evidently sinking. Takes nothing, and vomits frequently fluid of a dark coffee colour. He died at nine in the evening.

Post-mortem Examination.—On opening the abdomen, a quantity of thin feculent fluid escaped, and, on examination, an aperture nearly of the size of a shilling was observed in the small intestine, which lay deeply in the pelvis. A very beautiful attempt to circumscribe the extravasation had been made by nature, by the agglutination of the coils of small intestine one to another, and these again to the omentum and peritoneum lining the interior of the abdominal parietes. Thus the fecal matter was prevented spreading over the general cavity of the peritoneum. The entire extent of peritoneum had been inflamed, from the adhesions formed between adjacent parts, and in several places there were large flakes of fibrine, with some serous fluid effused. The gall-bladder was extremely turgid, with dark-green bile. The mucous membrane of the stomach exhibited numerous petechiæ, and in many points, particularly towards the cardiac extremity, patches of vascular network were observed. Bladder empty, its peritoneal surface forming part of the cavity which contained the fecal fluid.

CORRESPONDENTS.

A Constant Reader.—Certificates of attendance granted in Paris on lectures and hospital practice in that capital, are not received by candidates for the diplomas and licenses at the College of Surgeons and Apothecaries' Hall, London. A knowledge of medicine which is obtained on the continent is deemed *a contraband commodity* by our wise and disinterested medical rulers.

An Inquirer.—There is no such law.

We declined the insertion of Dr. Hope's letters in the body of THE LANCET, and we must therefore refer *Mr. J. Knight* to our publisher. Any other proceeding would be unfair to Dr. Hope.

The paragraph from Aldersgate-street, also, can only appear as an advertisement. We grudge not our space, but announcements of these presents and addresses are strictly advertisements, possessing interest solely for the donors and the receivers.

Mr. H. Brand errs in some measure in his judgment on the case. It may deserve remark, but not such strictures as Mr. B. desires to make. Two other instances of similar and successful treatment are now before us.

We beg to acknowledge the receipt of the communications of *Mr. Storrow*, *Mr. S. Johnson*, *Dr. Kelso*, *Dr. Fox*, and *Dr. Alexander*.

Dr. T.—"Were" is right. The verb is governed by "indications." Pardon the "obtained."

ERRATUM.—In Dr. Thacker's case of hydrocephalus, page 139, line 11, for *Russia* read *Prussia*.